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VOL. LXXI.

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PROCEEDINGS

OF

THE ROYAL SOCIETY.

"Magnetic Observations in Egypt, 1893—1901." By Captain H. G. LYONS, R.E. Communicated by Professor RÜCKER, Sec. R.S. Received June 6,—Read June 20, 1901.

The following Magnetic Observations have been made at various times during the years 1893 to 1899, at first with a Declinatorium, made by Bamberg, of Berlin, the property of the Egyptian Government, and later with a Kew Magnetometer, No. 73, and Dover's Dip Circle, No. 99, both kindly lent by the Council of the Royal Society on the recommendation of Professor A. W. Rücker, F.R.S. These observations are most conveniently divided into five groups, each of which includes observations made during a single period and with a single instrument—

I. Observations made with a Declinatorium by Bamberg, of Berlin:

(*a.*) In the neighbourhood of Cairo, 1893—1894.

(*b.*) In the Lybian Desert, near the Kharga and Dakhla Oases, in December, 1893, and January, 1894.

(*c.*) In the Lybian Desert from the Wadi Natrun to the Baharia Oasis, April, 1894.

II. Observations taken with Kew Magnetometer, No. 73, and Dover's Dip Circle, No. 99, in the Nile Valley from Cairo to the 2nd Cataract, November, 1894, to June, 1896.

(*a.*) Declination. (*b.*) Dip and Horizontal Force.

III. Observations taken to determine the Diurnal Variation of the Declination.

IV. Observations taken at Helwan, near Cairo, in November and December, 1898.

V. Determination of the Annual Variation from the above observations and those of various observers in previous years.

The declinatorium used at first consisted of a horizontal circle furnished with two verniers reading to 30″, while the magnet was balanced on a vertical steel pivot, in a box which occupied the centre of the horizontal circle. Attached to the magnet was a mirror, in which the reflected image of the cross wires was observed with a small telescope placed excentrically. The same telescope also served to observe the sun or a meridian mark for determining the geographical meridian.

The geographical positions were taken from Maps Nos. 740, 662, published by the Intelligence Division, War Office, or from astronomical observations taken on the spot; the former are indicated by * and the latter by †.

In each case the time of the observation is given as Cairo mean time, *i.e.*, $2^h\ 5^m\ 8^s{\cdot}9$ fast of Greenwich, since all the observations were made before the time of the 30° meridian E. of Greenwich was adopted as civil time for Egypt.

D'Abbadie's station, beside the Great Pyramid at Giza, was occupied on May 10, 1901, to obtain improved values of the secular variation. The observations were made between 1 P.M. and 4 P.M. when the electric tramway was not working.

Since there are, as yet, no self-registering magnetic instruments in Egypt, it is impossible to reduce the results obtained to a single epoch with any accuracy; they are, therefore, given as they were originally observed.

Helwan, 20 kiloms. south of Cairo, was chosen for the observations of 1898 and 1899, since the electric tramways of Cairo render observations impracticable even if the amount of iron in the present observatory building at Abbassia did not vitiate all observations taken there. For this reason the value of 5° 36′ west for the declination, given in the 'Bulletin Mensuel' of the Abbassia Observatory for June, 1886, is wholly wrong.

In observing with the Declinatorium, the feet of the tripod were firmly pressed into the ground, and this was found sufficient for the precision obtainable with the instrument. With the Kew pattern magnetometer, however, wooden pickets were driven firmly into the ground, and the feet of the tripod rested on these, thus avoiding any errors due to the tripod sinking into soft or sandy soil.

All the observations which follow may be considered as satisfactory ones taken under favourable conditions, since all those which were interfered with by high winds, sand storms, &c., have been omitted. The stations where igneous rocks are known to be near enough to affect the results somewhat are marked with an asterisk on page 10.

At several places on the Bahr el Abyad granite masses, and occasionally basaltic rocks, rise through the sandstone, and the high value for declination obtained at Renk (page 23) is probably due to this.

The times given are those of the middle of the observation, *i.e.*, for Girga—

		Mean.
Declination observation	8.58—9.11	9.5
First vibration	9.14—9.27	9.20
Second vibration...	9.34—9.47	9.40
Deflection	10.35—11.0	

The time of the horizontal force value is given as the middle of the time of the vibration observation.

Where one value is given for the dip one needle was used; where two values, the value obtained from each is given.

Deflection observations as well as the vibration observation were made on each occasion for the observations given in Table II(*b*); in the observations given on page 22 it is mentioned when it was not possible to take them.

I (*a*). Declination at Stations near Cairo determined with a Declinatorium.

Place.	Latitude N.	Longitude E.	Date.	Cairo mean time.	Declination West.	
	° ′ ″	° ′ ″		h. m.	°	
Mataria, site of ancient Temple of Heliopolis	*30 7 40	31 8 53	2-10-93	15 45	4 26·9	
Abbassia, The Polygon Camp....	*30 4 13	31 18 8	11-8-93	6 20	4 17·7	
„ „ „	„ „	„ „	11-10-93	16 10	4 24·6	
„ „ „	„ „	„ „	24-8-94	8 0	4 11·7	
„ „ „	„ „	„ „	26-10-94	7 0	4 17·0	
Cairo, Gezira Hut Barracks, S. end	*30 3 25	31 13 3	14-8-93	16 45	4 27·3	
Cairo, Venus Station on Jebel Moqattam	†30 1 46	31 16 39	17-8-93	18 20	4 35·9	Position determined by Transit of Venus Expedition, 1874.
Cairo, Moqattam Fort, south side	*30 1 31	31 16 6	20-9-93	16 58	4 24·6	
Mena House Hotel, Giza Pyramids	*29 59 30	31 7 46	1-9-93	15 15	4 30·4	
Saqqara, Mariette's House	*29 52 30	31 12 47	25-8-93	12 45	4 30·4	
Helwan, right bank of Nile.....	*29 51 32	31 20 16	13-10-93	13 30	4 27·7	
Mit Rahini Village, left bank of Nile	*29 50 57	31 15 3	25-8-93	8 0	4 19·5	

I (*b*). Declination at Stations in the Lybian Desert near Kharga and Dakha Oases, determined with a Declinatorium.

Place.	Latitude N.	Longitude E.	Date.	Cairo mean time.	Declination West.	
	° ′ ″	° ′ ″		h. m.	° ′	
Assiut, left bank of Nile	27 11 0	31 12 0	15-12-93	9 40	4 47·5	Position given by Jordan, 1874.
On caravan road between Assiut and Kharga Oasis	26 50 0	30 58 15	18-12-93	13 30	4 52·7	Position fixed by compass traverse between Assiut and Kharga.
Kharga village	†25 26 20	30 32 50	22-12-93	10 0	5 1·5	
Beris, Kharga Oasis.....	†24 42 33	30 36 0	27-12-93	17 0	4 54·8	
„ „ „	„ „	„ „	11-1-94	16 0	4 55·7	
On caravan road between Beris in Kharga Oasis and Mut in Dakhla Oasis	25 10 20	29 47 20	31-12-93	9 20	5 23·9	Position from compass traverse.
Mut, Dakhla Oasis	†25 29 4	29 4 40	2-1-94	17 0	5 22·8	
Point on caravan road south of Mut	24 16 45	29 14 30	7-1-94	14 30	5 32·0	Position from compass traverse from Mut to Beris.
Point in Desert west of Beris....	24 37 30	30 20 40	9-1-94	15 10	4 56·2	„ „
Point on Derb el Arbain caravan road south of Beris	23 52 30	30 27 40	13-1-94	15 30	5 14·2	„ „
Bir Murr	†23 19 30	30 11 40	15-1-94	9 5	5 3·2	

I (*c*). Declination at Stations in Wadi Natrun and Baharia Oasis, determined with a Declinatorium.

Wadi Natrun, Deir Anba Bishoi	†30 19 25	30 25 0	16-4-94	10 30	4 38·3	
„ „ Deir Baramus....	†30 21 18	30 16 24	17-4-94	16 0	4 54·0	
Baharia Oasis, Mandisha.....	†28 21 00	28 58 0	24-4-94	8 30	5 8·9	

Description of Stations observed at with Declinatorium.

Place.	Description.
Mataria	On ruins of Temenos wall of Temple of Heliopolis; S. of obelisk.
Abbassia	N. end of Polygon, E. of Suez road, E. end of row of trees behind battery.
Gezira.....	100 metres north of Grotto, N.W. of Gezira.
Venus Station, Jebel Moqattam.	Observation point of Transit of Venus Expedition, 1874.
Moqattam Fort	200 metres south of old fort on spur above Citadel, *not* the earthwork on the top of the hill by the Cholera Camp of 1883.
Mena House, Giza..	Opposite Mena House Hotel, S. of road to Pyramids.
Saqqara	150 metres N.E. of Mariette's house.
Helwan	20 kilom. S. of Cairo; on a low hill E. of water reservoirs to N.E. of town.
Mit Rahini.....	50 metres west of the larger Ramses Colossus on the site of Memphis.
Assiut	On bank 300 metres S. of railway station and 80 metres E. of main road to town.
Kharga	50 metres S. of blockhouse S.E. corner of village.
Beris	E. of village 100 metres E. of fort.
Mut..... ..	West end of the old Government buildings.
Bir Murr	Close to the well.
Deir Anba Bishoi ..	In garden between guest chamber and church.
Deir Baramus	On roof, east side of guest chamber.
Mandisha	In front of Omda's house.

II (*a*). Declination at Stations in the Nile Valley, determined with Kew pattern Magnetometer No. 73.

Place.	Latitude N.	Longitude E.	Date.	Cairo mean time.	Declination West.	
	° ′ ″	° ′ ″		h. m.	° ′	
Abbassia, Polygon Camp.....	*30 4 13	31 18 8	27-9-95	8 30	4 3·9	
„ „ „	„ „	„ „	11-6-96	9 30	4 5·4	
Girga, left bank of Nile	*26 20 10	31 53 25	24-11-94	9 5	4 29·3	
Dendera, left bank of Nile	*26 10 0	32 45 0	3-11-95	11 40	4 43·2	
Luxor, right bank of Nile	25 41 50	32 38 34	15-3-95	15 10	4 26·8	Position from Venus Transit Expedition, 1874.
Esna, left bank of Nile	25 18 0	32 34 0	13-3-95	12 55	4 51·8	Position from Jordan, 1874.
Edfu „ „	24 58 40	32 53 5	30-12-94	8 0	4 7·3	„ „ „
Assuan, Elephantine Island	†24 5 0	32 52 0	6-2-95	9 0	4 22·6	
1st Cataract, South end, Bab el Gedid	*24 2 30	32 51 0	9-3-95	11 0	4 33·1	
Philæ, Great Temple	†24 1 10	32 51 50	20-12-95	14 50	4 24·6	
„ „ „	„ „	„ „	11-4-96	10 7	4 22·9	
Dabod, left bank of Nile	†23 53 40	32 53 10	9-2-95	16 30	4 21·7	
Taifa „ „	†23 37 30	32 53 20	7-3-95	13 40	4 23·3	
Kalabsha „ „	†23 32 10	32 53 25	12-2-95	6 30	4 22·1	
Abu Hor, right bank of Nile	†23 28 15	32 55 0	13-2-95	15 20	4 29·0	
Gerf Hussein, left bank of Nile..	†23 17 20	32 56 25	15-2-95	9 30	4 27·2	
Dakka „ „ ..	†23 9 5	32 47 15	16-2-95	8 40	4 18·7	
Uffeduni „ „ ..	†23 0 54	32 42 12	17-2-95	8 30	4 27·6	
Sebua „ „ ..	†22 45 5	32 35 10	20-2-95	8 40	4 35·0	
Korosko, right bank of Nile	†22 36 38	32 18 5	30-11-94	9 20	4 35·9	
Amada, left bank of Nile.....	†22 43 30	32 16 30	5-3-95	14 0	4 37·9	
Abu Simbel, left bank of Nile....	†22 19 40	31 39 0	23-2-95	14 40	5 0·2	
Akshi (Serra) „ „	†22 8 0	31 20 9	2-3-95	10 10	5 25·4	
Wadi Halfa, right bank of Nile..	†21 54 49	31 19 2	26-2-95	11 40	4 39·9	
Amka (ancient fort Mirgissi) left bank of Nile	*21 48 30	31 11 0	28-2-95	12 20	4 55·7	
Murrat Wells, Nubian Desert....	†21 3 54	32 54 40	9-12-95	8 10	5 42·3	
Wadi Mogharin „ „	†21 20 35	32 20 0	14-12-95	17 20	3 48·6	

II (*b*). Dip and Horizontal Force in the Nile Valley.

Place.	Date.	Cairo mean time.	Dip.	Cairo mean time.	Horizontal force.		No. of dip needle.
		h. m.	° ′	h. m.			
Abbassia, Polygon Camp.....	3-5-95	12 10	40 44·0	10 20	0·30076	—	2
„ „ „	12-6-96	16 50	40 41·4	16 0	0·30038	Lat. 28° 38′ 50″	2
Maghagha.....	19-11-97	13 00	38 11·8	—	—	—	—
Girga, left bank of Nile.....	24-11-94	10 50	34 37·0	—	—	—	1
„ „ „	6-11-95	13 10	34 30·5	9 20	0·31608	—	1
		11 40	34 31·4	9 40	0·31622	—	2
Dendera „ „	5-11-94	11 40	34 18·5	—	—	—	1
		12 10	34 19·0				2
				11 15	0·31874		
„ „ „	7-11-95	—	—	11 45	0·31904	—	—
Luxor, right „	15-3-95	16 40	33 38·1	15 30	0·32070	—	2
Mualla „ „	14-3-95	6 30	32 58·7	—	—	Lat. 25° 29′ 0″	1
Edfu, left „	30-12-94	8 30	31 56·0	—	—	—	1
„ „ „	13-3-95	—	—	8 30	0·32357	—	—
Assuan, Rest Camp	29-12-94	11 20	30 19·7	7 15	0·32364	Lat. 24° 7′ 0″	1
„ „	11-3-95	17 20	30 18·8	—	—	—	1
		16 20	30 18·2				2
Philæ Island	9-3-95	7 30	30 28·9	18 15	0·32440	—	1
		8 5	30 29·1				2
Dabod, left bank of Nile.....	9-2-95	—	—	16 40	0·32390	—	—
„ „ „	10-2-95	7 10	30 16·5	—	—	—	1
		7 45	30 16·3				2
Taifa „ „	7-3-95	10 25	29 46·7	13 30	0·32716	—	1
		9 50	29 46·7				2
Kalabsha „ „	11-2-95	17 40	29 26·4	7 0	0·32926	—	1
		17 0	29 26·0				2
Abu Hor, right „	13-2-95	17 30	29 34·2	15 40	0·32702	—	1
Gerf Hussein, left bank of Nile ..	15-2-95	15 30	29 1·5	12 20	0·32815	—	1
		14 50	29 8·3				2

II (*b*)—*continued.*

Place.	Date.	Cairo mean time.	Dip.	Cairo mean time.	Horizontal force.	—	No of Dip Needle.
		h. m.	° ′	h. m.			
Dakka, left bank of Nile.....	16-2-95	14 10	29 10·6	12 0	0·32795	—	1
		13 40	29 10·6				2
Uffeduni „ „	17-2-95	16 0	28 35·2	12 40	0·32902	—	1
		15 30	28 35·2				2
Siala, right „	26-12-94	13 20	28 27·0	7 30	0·32747	—	1
		13 35	28 28·4				2
Sebua, left „	19-2-95	14 40	28 19·8	—	—	—	1
„ „ „		15 30	28 17·8				2
	20-2-95	—	—	11 50	0·32932	—	—
Korosko, right bank of Nile.....	1-12-94	16 40	28 6·7	15 0	0·32692	—	1
		17 10	28 5·2				2
„ „ „	21-2-95	—	—	16 0	0·32638	—	—
Amada, left „	5-3-95	17 14	27 47·7	14 20	0·32908	—	1
		17 10	27 47·0				2
Abu Simbel, left bank of Nile....	23-2-95	6 40	27 15·4	17 0	0·32735	—	1
		7 10	27 16·5				2
Akshi (Serra), left bank of Nile ..	2-3-95	6 50	26 51·0	10 0	0·33316	—	1
		7 30	26 49·4				2
Wadi Halfa, right „ ..	25-2-95	17 30	26 28·1	—	—	—	1
„ „ „ „ ..	27-2-95	17 30	26 28·6	14 50	0·32939	—	1
		16 50	26 28·4				2
Bir Ongat, Nubian Desert	20-12-94	11 25	26 18·1	7 0	0·33148	—	1
		12 0	26 23·5	7 15	0·33149	—	2
Wadi Mogharin „	14-12-94	16 30	25 23·7	—	—	—	1
		17 10	25 22·4				2
Murrat Wells	11-12-94	15 40	24 27·1	—	—	—	1
		16 20	24 27·6				2
„ „	12-12-94	—	—	8 15	0·33695	—	—

Description of Stations observed at with Magnetometer and Dip Circle.

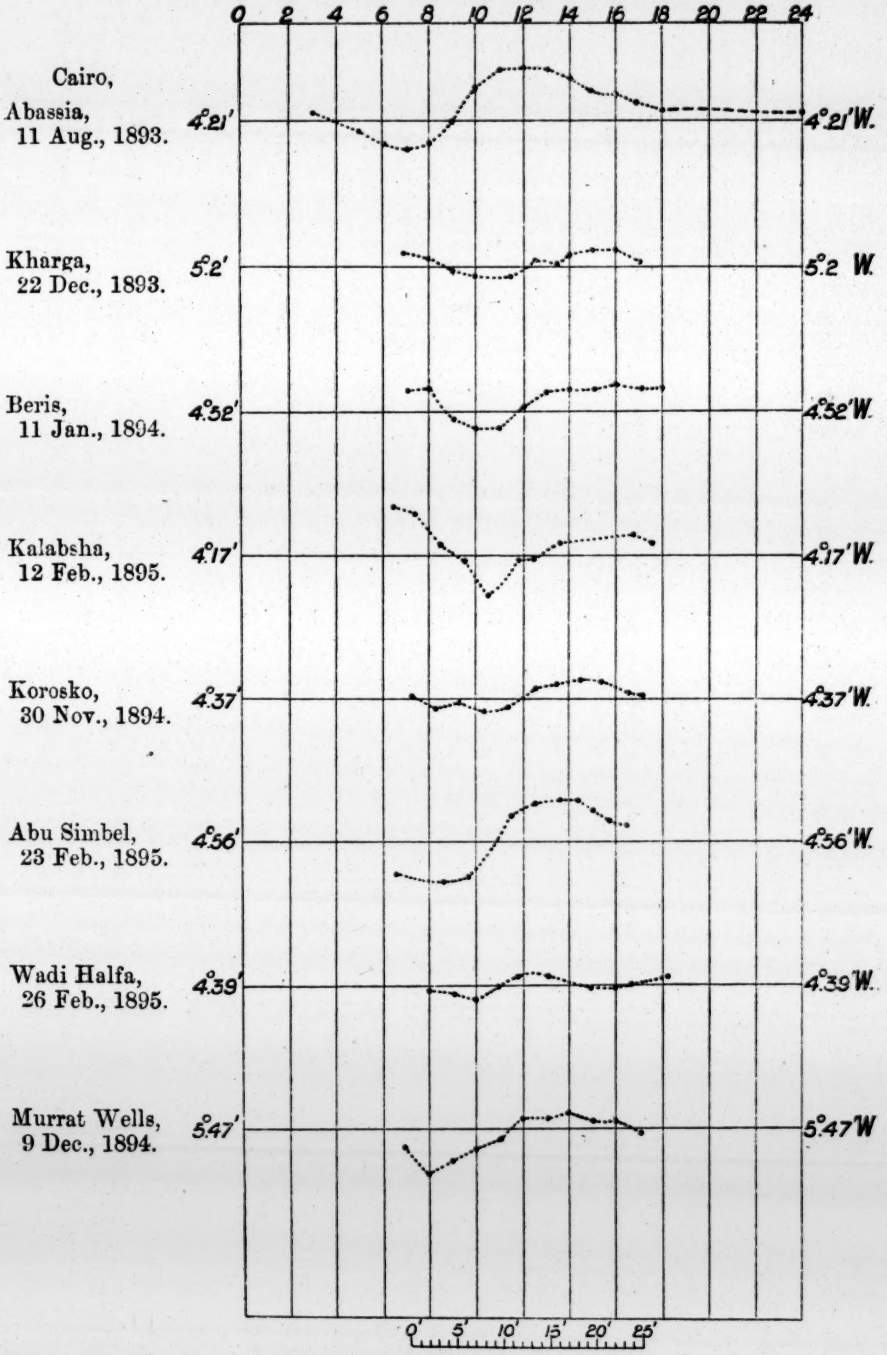
Place.	Description.
Abbassia.....	The same as for Declinatorium.
Girga	S.W. of huts, which are 150 metres west of canal head on N. side of steamer landing barge.
Dendera	100 metres E. of point where steamers stop.
Luxor	On river bank, 50 metres north of house painted red and white, on bank south of Luxor.
Mualla	2 kiloms. S. of irrigation resthouse.
Esna	100 metres north of landing barge, and on river bank.
Edfu	40 metres from bank, and 120 metres S. of landing barge.
Assuan*	Rest camp, S. side of 2nd hut from N.W. corner.
Elephantine*.....	25 metres S.E. from gateway of ancient temple.
1st Cataract*.....	South side of the Bab el Gedid.
Philæ*.....	Till 29 Nov. E. side of kiosk, on quay, after then on roof of Temple of Isis, N. end.
Dabod.....	200 metres N. of ancient masonry quay.
Taifa*.....	100 metres from S.E. corner of village, on river bank.
Kalabsha*	30 metres from river bank, and 70 metres north of great temple.
Abu Hor*	On river bank, 100 metres S. of steamers' stopping place.
Gerf Hussein	At entrance to temple.
Dakka.....	S.E. angle of temple.
Uffeduni	On eastern part of temple ruins.
Siala	On bank opposite N. end of village.
Sebua	On river bank 50 metres S. of line of temple axis.
Bir Ongat*.....	Close to the well.
Wadi Mogharin*...	At mouth of Wadi, to W. of Murrat Wells.
Murrat Wells,* Nubian Desert.....	On rocky spur between the two blockhouses.
Korosko	On river bank, S.W. end of officers' quarters.
Amada	150 metres S.E. of temple, and on the foundation course of an ancient building.
Abu Simbel	50 metres S.E. of and slightly lower than great temple entrance.
Akshi (Serra).....	West bank, 200 metres S. of temple ruins.
Wadi Halfa.....	Under gamaiza tree, 60 metres S. of Commandant's house.
Mirgissi Fort*.....	In ancient Egyptian fort W. of remains of small temple in N.W. corner of the walled enclosure.
Great Pyramid, Giza	10.5.1901 occupied d'Abbadie's station close to pyramid, east of it; on ruins of the most northern of three small pyramids.

* Denotes crystalline rocks in the immediate neighbourhood.

III. *Diurnal Variation of the Magnetic Declination.*

In the absence of any self-registering apparatus in Egypt, the diurnal variation of the declination has been observed hourly at a few places, and the results of some of these observations are here given graphically. Except in the case of Abbassia, a single day only was available.

Curves of Diurnal Variation of Declination.



Scale of twenty-five minutes of arc.

Abbassia, Kharga, and Beris observed with Declinatorium, the rest with Kew pattern magnetometer.

W. 4° +.

Mean Values of Diurnal Variation of δ at Philæ.

Period.	7	8	9	10	11	Noon.	1	2	3	4	5
	′	′	′	′	′	′	′	′	′	′	′
Dec. 23—Dec. 31	26·1	24·9	25·4	25·8	27·4	27·3	—*	26·1	25·8	26·0	26·2
Jan. 1—Jan. 10†	26·6	26·7	26·5	27·6	28·7	28·6	27·4	27·1	26·9	26·6	27·2*
Jan. 11—Jan. 20.....	26·1	25·5*	25·4	25·9	27·3	27·7*	26·2	25·5*	25·2	25·9	25·8*
Jan. 21—Jan. 31‡	25·5*	26·0	24·9	24·8	25·1	26·1	25·6*	26·8*	27·0	26·7	26·2*
Feb. 1—Feb. 10	26·2*	25·5	25·7	25·3	26·3	26·6	26·1	26·2	25·8	25·5	26·4*
Feb. 11—Feb. 20§	26·2*	25·2	25·0	24·9	25·7	26·5	26·6*	26·6*	25·6	24·9	—
Feb. 21—Feb. 29‖	—	24·0	23·4	23·2	23·9	24·6	24·6	—	24·4	23·5	—
March 1—March 10¶.....	—	24·9	25·8	26·7	27·6	27·3	27·9	25·8*	—	25·1	—
March 11—March 20.....	—	23·7	24·6	25·7*	26·2	27·7	27·6	26·3	25·3*	25·6	24·7*
March 21—March 31.....	21·8	21·8	22·0	23·9	25·8	27·4	26·5*	26·4	24·4*	24·4	23·4
April 1—April 10	22·8*	21·0	23·3*	23·0*	24·9	25·1	—	25·2	—	24·6*	22·3

* Too few readings to give a fair average.
† No observations 8th and 9th Jan.
‡ No observations 21st, 25th, 30th, and 31st Jan.
§ Observations few 11th—16th Feb.
‖ Observations on 23rd and 25th Feb. only.
¶ No observations on 1st, 2nd, 3rd, and 6th March.

W. 4° +

Hours.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
7	26·8	28·2	—	27·1	25·5	26·4	25·7	No observations.	No observations.	—	26·4
8	25·3	28·0	27·8	26·9	25·7	—	—			26·4	26·2
9	24·6	26·2	26·8	—	—	26·6	—			—	26·4
10	26·6	27·7	26·8	28·6	28·4	28·0	28·4			26·2	27·7
11	26·9	28·6	—	28·4	30·0	29·6	30·0			28·4	28·2
Noon	27·1	28·0	29·8	27·8	29·5	—	28·4			29·8	—
1	—	27·7	28·2	—	26·4	28·4	—			—	—
2	27·1	—	28·2	26·0	—	27·3	—			—	26·6
3	28·0	27·5	27·1	26·2	—	—	26·4			26·2	27·1
4	27·8	—	28·0	24·6	26·0	26·6	—			—	26·9
5	27·5	—	—	27·8	—	26·2	—			—	26·4

W. 4° +

Hours.	1.	2.	3.	4.	5.	6.	7.	8.	9.
7	—	—	No observations.	26·2	—	—	—	—	—
8	—	27·7		25·7	24·6	25·7	24·9	25·5	25·
9	26·9	26·8		25·7	—	24·6	24·9	26·2	24·
10	—	—		25·1	25·1	24·6	25·8	26·9	24·
11	26·2	26·2		25·1	26·2	25·3	27·7	27·3	26·
Noon	—	26·4		26·8	26·4	26·4	27·7	27·8	26·
1	25·7	—		26·6	26·6	26·4	—	26·6	—
2	—	—		26·9	26·4	25·7	26·6	26·4	25·
3	26·2	—		—	—	26·4	—	—	25·
4	26·2	—		26·2	25·1	24·8	25·3	26·2	25·
5	—	—		—	—	—	—	26·4	—

W. 4° +

Hours.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.
7	—	—	—	—	—	—	—	—	—	—	—	—
8	—	—	—	24·6	24·4	—	25·3	24·9	24·6	25·5	23·9	24·6
9	—	—	—	25·8	25·5	—	25·5	26·2	25·7	26·4	24·6	24·9
10	—	—	—	27·3	26·8	—	—	26·2	26·2	26·9	25·7	—
11	—	—	—	28 2	27·7	—	26·8	27·3	—	28·2	—	26·8
Noon	—	—	—	27·8	—	—	26·6	27·7	26·4	28·0	28·2	27·5
1	—	—	—	29 3	—	—	27·5	26·4	28·2	28·2	27·3	27·5
2	—	—	—	—	25·8	—	—	—	—	—	—	26·6
3	—	—	—	—	—	—	—	—	—	—	26·2	—
4	—	—	—	24·6	24·1	—	24·6	24·6	26·4	26·4	26·2	25·7
5	—	—	—	—	—	—	—	—	—	—	—	—

January, 1896.

11.	12.	13.	14.	15.	16.	17.	18.	19.	20.	21.	22.	23.	24.	25.
26·4	26·2				—	26·6	—	26·4	24·6		25·5	—	—	
26·2	—				—	—	24·9	—	—		25·1	26·2	—	
26·4	26·2				24·6	25·5	24·6	26·4	24·2		—	24·8	24·1	
27·7	25·3				24·4	26·6	26·4	—	24·8		25·1	24·6	—	
28·2	26·0				26·6	—	28·0	29·3	25·7		—	26·4	24·6	
—	27·8				27·5	27·7	—	—	—		27·1	28·2	24·9	
—	26·6				25·7	—	26·4	—	26·2		—	—	—	
26·6	24·6				—	—	25·3	—	—		26·2	—	—	
27·1	25·3				24·6	24·8	—	24·6	24·6		—	27·8	26·2	
26·9	26·4				25·5	—	24·8	—	—		—	27·1	—	
26·4	—				—	—	—	—	24·4		26·2	—	26·2	

No observations. | No observations. | No observations. | No observations.

February, 1896.

9.	10.	11.	12.	13.	14.	15.	16.	17.	18.	19.	20.	21.
—	—	—		—		—		26·2	—	—	—	—
25·3	24·6	25·1		26·4		—		—	25·3	24·8	24·4	—
24·6	—	24·4		26·4		25·3		24·8	24·6	24·8	24·9	—
24·6	24·8	24·6		24·6		—		—	24·6	24·6	26·2	—
26·4	26·2	—		24·1		26·2		24·9	25·1	26·6	27·5	—
26·4	25·3	—		25·3		26·4		—	26·6	27·7	—	—
—	24·8	—		—		—		24·8	27·1	27·8	—	—
25·3	—	—		—		—		26·8	—	26·6	26·4	—
25·7	24·8	—		24·8		—		26·4	26·4	—	24·9	—
25·5	24·6	—		24·6		—		—	25·7	24·8	24·6	—
—	—	—		—		—		—	—	—	—	—

No observations. | No observations.

March, 1896.

12.	13.	14.	15.	16.	17.	18.	19.	20.	21.	22.	23.	24.	25.
—	—	—	—	—	—	—	—	—	—	23·8	—	—	21·
24·6	—	22·8	23·7	23·3	23·5	23·3	24·2	—	21·7	22·8	—	—	22·
24·9	—	23·8	24·8	—	24·6	24·0	25·3	—	21·9	21·7	—	—	—
—	—	—	—	—	—	—	—	—	—	22·3	—	—	26·
26·8	—	24·6	—	24·6	26·6	27·1	27·3	—	27·1	23·7	—	—	—
27·5	—	26·4	26·4	—	27·7	28·2	28·2	—	28·2	25·1	—	—	29·
27·5	—	—	—	—	28·2	—	27·3	—	—	26·4	—	—	—
26·6	—	25·7	—	27·5	—	26·4	25·5	—	26·4	25·8	—	—	27·
—	—	—	—	25·5	26·2	—	—	—	—	24·6	—	—	—
25·7	—	25·5	—	—	—	25·7	25·1	—	25·7	23 1	—	—	24·
—	—	—	—	—	24·6	24·8	—	—	24·8	23·7	—	—	—

January, 1896.

	6.	7.	8.	9.	10.	11.	12.	13.	14.	15.	16.	17.	18.	19.	20.	21.
·5	26·4	25·7			—	26·4	26·2				—	26·6	—	26·4	24·6	
·7	—	—			26·4	26·2	—				—	—	24·9	—	—	
—	26·6	—			—	26·4	26·2				24·6	25·5	24·6	26·4	24·2	
·4	28·0	28·4			26·2	27·7	25·3				24·4	26·6	26·4	—	24·8	
·0	29·6	30·0			28·4	28·2	26·0				26·6	—	28·0	29·3	25·7	
·5	—	28·4			29·8	—	27·8				27·5	27·7	—	—	—	
·4	28·4	—			—	—	26·6				25·7	—	26·4	—	26·2	
—	27·3	—			—	26·6	24·6				—	—	25·3	—	—	
—	—	26·4			26·2	27·1	25·3				24·6	24·8	—	24·6	24·6	
·0	26·6	—			—	26·9	26·4				25·5	—	24·8	—	—	
—	26·2	—			—	26·4	—				—	—	—	—	24·4	

No observations.

February, 1896.

	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	14.	15.	16.	17.	18
	26·2	—	—	—	—	—	—	—		—		—		26·2	—
	25·7	24·6	25·7	24·9	25·5	25·3	24·6	25·1		26·4		—		—	25
	25·7	—	24·6	24·9	26·2	24·6	—	24·4		26·4		25·3		24·8	24
	25·1	25·1	24·6	25·8	26·9	24·6	24·8	24·6		24·6		—		—	24
	25·1	26·2	25·3	27·7	27·3	26·4	26·2	—		24·1		26·2		24·9	25
	26·8	26·4	26·4	27·7	27·8	26·4	25·3	—		25·3		26·4		—	26
	26·6	26·6	26·4	—	26·6	—	24·8	—		—		—		24·8	27
	26·9	26·4	25·7	26·6	26·4	25·3	—	—		—		—		26·8	—
	—	—	26·4	—	—	25·7	24·8	—		24·8		—		26·4	26
	26·2	25·1	24·8	25·3	26·2	25·5	24·6	—		24·6		—		—	25
	—	—	—	—	26·4	—	—	—		—		—		—	—

No observations.

No observations.

No observations.

March, 1896.

7.	8.	9.	10.	11.	12.	13.	14.	15.	16.	17.	18.	19.	20.	21.
—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
25·3	24·9	24·6	25·5	23·9	24·6	—	22·8	23·7	23·3	23·5	23·3	24·2	—	21·
25·5	26·2	25·7	26·4	24·6	24·9	—	23·8	24·8	—	24·6	24·0	25·3	—	21·
—	26·2	26·2	26·9	25·7	—	—	—	—	—	—	—	—	—	—
26·8	27·3	—	28·2	—	26·8	—	24·6	—	24·6	26·6	27·1	27·3	—	27·
26·6	27·7	26·4	28·0	28·2	27·5	—	26·4	26·4	—	27·7	28·2	28·2	—	28·
27·5	26·4	28·2	28·2	27·3	27·5	—	—	—	—	28·2	—	27·3	—	—
—	—	—	—	—	26·6	—	25·7	—	27·5	—	26·4	25·5	—	26·
—	—	—	—	26·2	—	—	—	—	25·5	26·2	—	—	—	—
24·6	24·6	26·4	26·4	26·2	25·7	—	25·5	—	—	—	25·7	25·1	—	25·
—	—	—	—	—	—	—	—	—	—	24·6	24·8	—	—	24·

0.	21.	22.	23.	24.	25.	26.	27.	28.	29.	30.	31.
·6		25·5	—	—		—	—	—	—		
—		25·1	26·2	—		26·2	26·2	26·2	—		
·2		—	24·8	24·1		24·6	24·9	25·1	25·7		
·8		25·1	24·6	—		24·4	24·6	—	25·5		
·7		—	26·4	24·6		24·6	24·6	25·1	—		
—		27·1	28·2	24·9		26·2	24·8	25·1	26·6		
·2		—	—	—		25·5	26·4	24·8	—		
—		26·2	—	—		—	26·6	—	27·8		
·6	No observations.	—	27·8	26·2	No observations.	26·2	—	—	27·7	No observations.	No observations.
—		—	27·1	—		26·6	26·8	26·0	27·1		
·4		26·2	—	26·2		—	—	—	—		

17.	18.	19.	20.	21.	22.	23.	24.	25.
6·2	—	—	—	—	—	—	—	—
—	25·3	24·8	24·4	—	—	24·8	—	23·3
4·8	24·6	24·8	24·9	—	—	23·2	—	23·7
—	24·6	24·6	26·2	—	—	22·1	—	24·4
4·9	25·1	26·6	27·5	—	—	23·0	—	24·8
—	26·6	27·7	—	—	—	24·6	—	24·6
4·8	27·1	27·8	—	—	—	—	—	24·6
6·8	—	26·6	26·4	—	—	—	—	—
6·4	26·4	—	24·9	—	—	—	—	24·4
—	25·7	24·8	24·6	—	—	—	—	23·5
—	—	—	—	—	—	—	—	—

.	21.	22.	23.	24.	25.	26.	27.	28.	29.	30.	31.
	—	23·8	—	—	21·9	20·5	—	21·2	—	—	—
	21·7	22·8	—	—	22·1	22·4	22·6	—	20·5	21·6	21·0
	21·9	21·7	—	—	—	23·1	—	21·9	—	21·6	—
	—	22·3	—	—	26·2	23·1	25·3	—	22·8	22·9	24·4
	27·1	23·7	—	—	—	25·8	26·6	24·6	—	24·6	26·4
	28·2	25·1	—	—	29·4	26·4	28·2	25·5	27·7	28·2	27·5
	—	26·4	—	—	—	26·4	—	—	—	26·8	—
	26·4	25·8	—	—	27·7	—	26·2	25·8	27·1	—	26·0
	—	24·6	—	—	—	—	24·2	—	—	—	—
	25·7	23 1	—	—	24·8	24·6	24·0	—	—	—	—
	24·8	23·7	—	—	—	—	—	23·1	22·6	23·5	22·8

W. 4° +

Hours.	1.	2.	3.	4.	5.	6.	7.	8.
7	26·8	28·2	—	27·1	25·5	26·4	25·7	No observations.
8	25·3	28·0	27·8	26·9	25·7	—	—	
9	24·6	26·2	26·8	—	—	26·6	—	
10	26·6	27·7	26·8	28·6	28·4	28·0	28·4	
11	26·9	28·6	—	28·4	30·0	29·6	30·0	
Noon	27·1	28·0	29·8	27·8	29·5	—	28·4	
1	—	27·7	28·2	—	26·4	28·4	—	
2	27·1	—	28·2	26·0	—	27·3	—	
3	28·0	27·5	27·1	26·2	—	—	26·4	
4	27·8	—	28·0	24·6	26·0	26·6	—	
5	27·5	—	—	27·8	—	26·2	—	

W. 4° +

Hours.	1.	2.	3.	4.	5.	6.
7	—	—	No observations.	26·2	—	—
8	—	27·7		25·7	24·6	25·7
9	26·9	26·8		25·7	—	24·6
10	—	—		25·1	25·1	24·6
11	26·2	26·2		25·1	26·2	25·3
Noon	—	26·4		26·8	26·4	26·4
1	25·7	—		26·6	26·6	26·4
2	—	—		26·9	26·4	25·7
3	26·2	—		—	—	26·4
4	26·2	—		26·2	25·1	24·8
5	—	—		—	—	—

W. 4° +

Hours.	1.	2.	3.	4.	5.	6.	7.	8.	9.	
7	—	—	—	—	—	—	—	—	—	
8	—	—	—	24·6	24·4	—	25·3	24·9	24·6	2
9	—	—	—	25·8	25·5	—	25·5	26·2	25·7	2
10	—	—	—	27·3	26·8	—	—	26·2	26·2	2
11	—	—	—	28 2	27·7	—	26·8	27·3	—	2
Noon	—	—	—	27·8	—	—	26·6	27·7	26·4	2
1	—	—	—	29 3	—	—	27·5	26·4	28·2	2
2	—	—	—	—	25·8	—	—	—	—	
3	—	—	—	—	—	—	—	—	—	
4	—	—	—	24·6	24·1	—	24·6	24·6	26·4	2
5	—	—	—	—	—	—	—	—	—	

Diurnal Variation of Declination at Philæ, Assuan.

Lat. N. 24° 1′ 10″; Long. E. 32° 51′ 50″, from December, 1895, to April, 1896.

While employed on Philæ Island examining and restoring the ancient buildings on the island, magnetic observations in the neighbouring country could not be undertaken for want of time. A magnetometer was set up, therefore, in a small, empty chamber on the roof of the Great Isis Temple, and the declination observed hourly from 7 A.M. to 5 P.M., except when the work on the excavations prevented. The results are given in the following tables (see also p. 12):—

December, 1895.

Hours.	20.	21.	22.	23.	24.	25.	26.	27.	28.	29.	30.	31.
7	—	26·9	26·0	26·4	26·4	25·7	—	26·0	No observations.	25·5	26·8	26·2
8	24·6	—	—	—	25·9	25·5	—	24·4		24·4	24·8	—
9	24·1	25·1	25·3	25·9	25·9	26·8	24·8	23·7		24·8	27·8	25·0
10	24·6	24·8	24·6	27·6	—	—	27·1	25·0		26·6	—	—
11	25·1	25·0	—	29·6	28·0	29·3	27·8	25·2		27·7	27·8	28·4
Noon	25·1	—	26·0	29·4	28·2	28·8	27·6	24·1		27·5	—	28·6
1	—	24·6	—	—	—	—	—	23·7		—	—	—
2	24·6	25·1	24·8	27·7	26·0	26·4	26·2	—		26·2	—	27·7
3	24·6	—	—	25·8	24·2	26·0	25·7	23·7		26·6	27·7	28·1
4	26·9	24·6	—	25·5	—	—	25·7	24·6		26·8	—	28·1
5	26·7	25·8	24·6	26·2	24·4	—	—	—		26·9	26·8	27·9

(See folding tables for months of January, February and March.)

W. 4° + April, 1896.

Hours.	1.	2.	3.	4.	5.	6.	7.	8.	9.
7	—	22·8	—	—	—	—	—	—	—
8	22·3	—	22·8	21·5	21·0	19·9	20·5	20·8	19·2
9	—	24·4	—	22·3	—	—	—	—	—
10	—	26·6	—	—	—	—	21·5	—	20·9
11	24·6	28·0	27·1	—	23·7	23·5	24·6	—	22·8
Noon	25·5	—	—	25·5	24·9	24·8	26·0	24·6	24·6
1	—	—	—	—	—	—	—	—	—
2	24·6	28·2	26·0	24·6	24·6	24·2	25·8	26·0	22·9
3	—	—	—	—	—	—	—	—	—
4	—	24·6	—	—	—	—	—	—	—
5	22·9	—	22·8	22·1	22·3	21·0	22·8	21·9	22·6

IV. *Magnetic Observations at Helwan, near Cairo.*

(See table on opposite page.)

V. *Secular Variation.*

As many early observations as it has been possible to find in various works have been collected in the following tables for the purpose of determining the average annual rate of change. Generally the declination appears to have been annually decreasing by about 6′ to 7′ in the first half of the century, becoming, however, 3′ to 4′ only in the second half. For the dip the available observations are very few; but from those at Alexandria and at the Great Pyramid, Giza, the rate of decrease appears to be about 1′ to 1′·5 annually. For the horizontal force the observations are too few, and give results which are not very concordant.

The observations before June, 1894, were made with the declinatorium, so that those made at later dates are more reliable, since a Kew pattern magnetometer with a unifilar suspension was used.

The values obtained cannot be considered as laying claim to a high degree of accuracy, seeing that in most cases the hour of observation is not given in the older observations; still in most cases the number of years elapsed is sufficiently great to reduce the error thus introduced into the value for the annual change to small dimensions.

(Tables are printed on pp. 16, 17 and 18.)

IV. Magnetic Observations at Helwan, near Cairo, 1898.

Date.	Time.	Dip.	Time.	Horizontal force.	Time.	Declination.	Notes.
	h. m.	° ′	h. m.		h. m.	° ′ ″	
October 27	15 42	40 31·6(1)*	—	—	—	—	(1) Dip needle 1.
November 1	14 50	0 31·2(1)*	—	—	—	—	(2) „ „ 3.
December 9	13 37	0 32·0(1)	—	—	12 35	4 15 45	(3) Uncorrected for torsion. This correction was almost invariably positive.
„ 9	14 17	0 27·8(2)	—	—	—	—	
„ 14	15 44	0 27·9(2)	14 21	0 30182	13 53	4 15 13	
„ 24	15 33	0 28·1(2)	14 10	0·30211	13 54	4 14 46	
„ 29	14 37	0 30·8(2)*	—	—	16 52	4 13 23(3)	
Mean values for 1899	—	40 32·1	—	0·30132	—	4 11 17	

The details of the above observations are given in the Meteorological Report of the Survey Department, Cairo, for 1898 and 1899.

* Observed by Mr. J. J. Craig.

V. Secular Variation of Declination.

Place.	Observer.	Date.	Declination west.			Change per annum.	
			°	′	″	′	
Alexandria	Quesnot and Nouet.	1798	13	6	0	—	
„	?	1842	8	24	0	−6·4	
„	Güssfeldt.	1876	5	55	9	−4·4	
„	Capt. Leslie, R.E.	1890	5	3	0	−3·8	
„ (Ramleh)	d'Abbadie.	1884	5	6	2	—	
Cairo	Russegger.	1839	9	2	0	—	
„ (Old Cairo) ..	d'Abbadie.	1885	5 5	15 28	9 3	−4·8	
„ Abbassia	H.G.L.	1895	4	5	0	−7·7	'39-95 = 5′·3
„ J. Moqattam..	d'Abbadie.	1885	4	56	5	—	
	H.G.L.	1893	4	36	0	−2·5	
„ Great Pyramid	d'Abbadie.	1885	4	48	9	—	
„ „ „	H.G.L.	1893	4	32	0	−2·1	
„ „ „	H.G.L.	1901	3	48	6	−5·5	'85-01 = 3′·8
Helwan	H.G.L.	1893	4	27	7	—	
„	Mean for 1899.	1899	4	11	3	−2·7	
Siwa Oasis	Cailliand.	1819	12	30	0	—	
„ „	Jordan.	1874	7	33	0	−5·4	
Mandisha (Baharia Oasis)	Cailliand.	1819	12	11	0	—	Village Zubbo
	Jordan.	1874	6	34	8	−6·1	„ Qasr
	H.G.L.	1894	5	8	9	−4·3	
Assiut	Cailliand.	1819	12	0	0	—	
	Jordan.	1874	5	42	0	−6·9	
	d'Abbadie.	1885	5	45	9	—	
	H.G.L.	1893	4	49	0	−2·7	1874-93
Kharga (Kharga Oasis)	Cailliand.	1819	12	10	0	—	
	Jordan.	1874	6	24	0	−6·4	
	H.G.L.	1894	5	3	0	−4·0	
	*Dr. J. Ball.	1899	4	15	0	−9·6	
Mut (Dakhla Oasis)	Cailliand.	1819	12	0	0	—	Qasr Dakhel, 20 kiloms. north of Mut
	Jordan.	1874	6	33	0	−6·0	
	H.G.L.	1894	5	24	0	−3·4	
Luxor	Cailliand.	1819	12	0	0	—	
	d'Abbadie.	1885	4 4	25 45	0 0	−6·5 −6·9	At Gurna on west bank
	H.G.L.		4	27	0	−6·0	

* Used Bamberg's Declinatorium.

V. Secular Variation of Declination—*continued.*

Place.	Observer.	Date.	Declination west.			Change per annum.	
			°	′	″		
Assuan	Cailliand.	1819	12	0	0	—	
	d'Abbadie.	1885	5	13	2	−6·1	
	H.G.L.	1895	4	26	0	−4·7	
Amada.....	Cailliand.	1819	11	13	0	—	At Tomas, 6 kiloms. up-stream
	H.G.L.	1895	4	38	0	−5·2	
Wadi Halfa.....	Cailliand.	1819	11	30	0	—	
	H.G.L.	1895	4	40	0	−5·4	
Mirgissi (Amka) ..	Cailliand.	1819	13	0	0	—	At Sarras, 20 kiloms. south
	H.G.L.	1895	4	56	0	−6·2	

The magnetic bearing of the walls or axes of certain temples is given in the "Description de l'Égypte," and from these an approximate value for the secular change in the declination may be deduced.

On Philæ—

	Magnetic bearing, 1799.	True bearing.	Declination, 1799.
Great Isis Temple.....	44° E. of N.	32° 5′ E. of N.	11° 55′
Nectanebo's Temple....	25° „	13° 28′ „	11° 32′
The Kiosk (or Pharaoh's Bed).....	104° 30′ „	93° 25′ „	11° 5′
		Mean.....	11° 31′

The Declination in 1896 was 4° 23′ W., which gives an average annual decrease of 4′·4.

Kom Ombo Temple—

Magnetic bearing of axis, 1799.....	55°	E. of N.
„ „ 1892.....	43° 55′	„
Change in 93 years	11° 5′	
„ per annum	7′·2	

Edfu Temple—

Magnetic bearing of axis, 1799	15°	E. of N.
„ „ 1892	3° 10′	„
Change in 93 years	11° 50′	
„ per annum	7′·7	

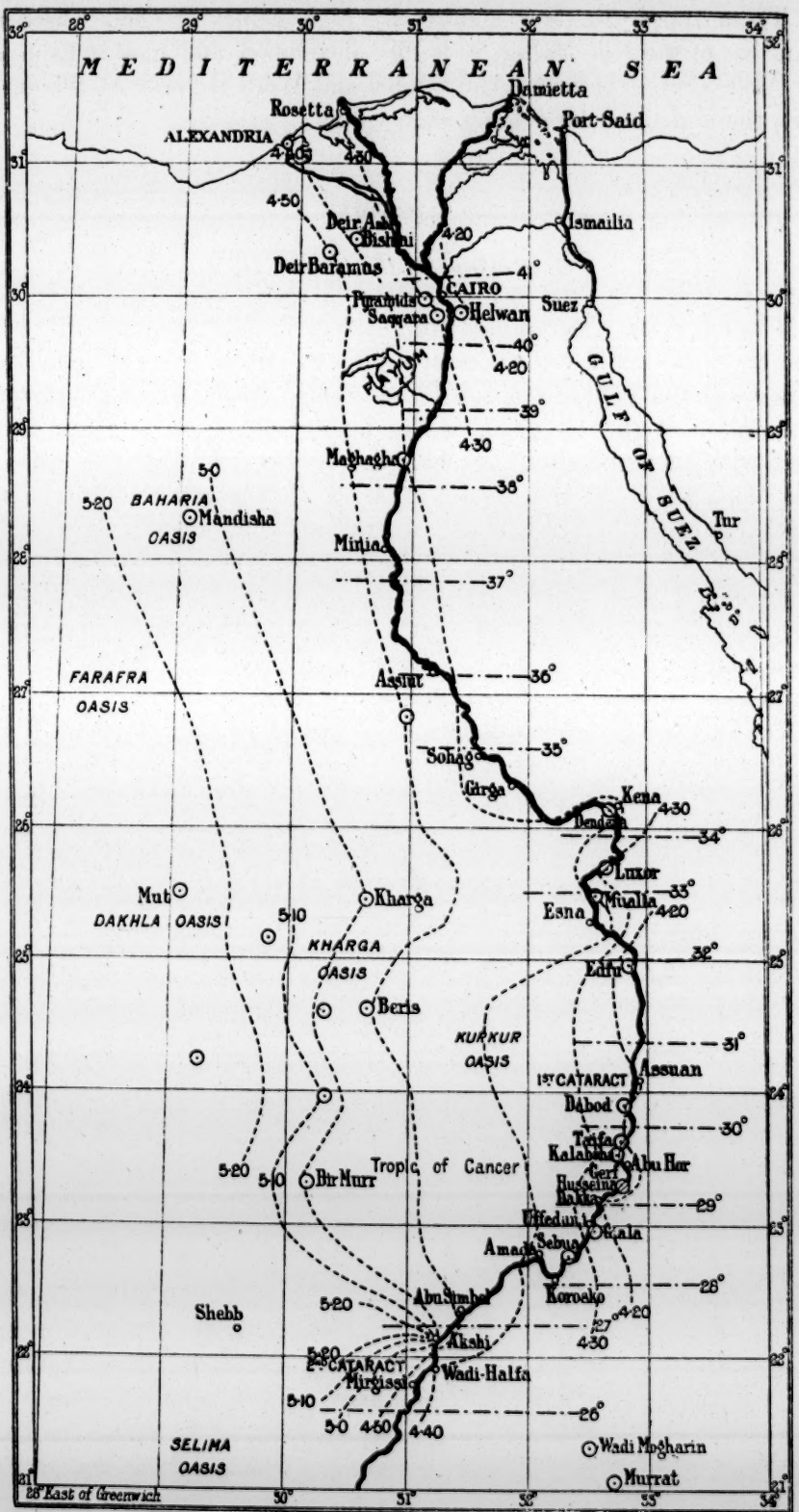
Secular Variation of Dip.

Place.	Observer.	Date.	Dip.	Yearly change.
			° ′	′
Alexandria	Quesnot and Nouet.	1798	47 30	—
„	?	1842	43 48	−5·0
„	Güssfeldt.	1876	42 52	−1·6
„ (Ramleh)	d'Abbadie.	1884	42 47·7	−0·7
Cairo—				
Great Pyramid....	d'Abbadie.	1839	41 41·8	—
„ „	„	1885	40 46·8	−1·2
	H.G.L.	1901	40 31·6	−1·0
Luxor	d'Abbadie.	1885	33 7·7	
	H.G.L.	1895	33 38·1	+3·0
Assuan	—	—	—	—

Secular Variation of Horizontal Force.

Place.	Observer.	Date.	H. in C.G.S. units.	Annual change.
Alexandria	—	1842–5	0·27955	—
„	Güssfeldt.	1876	0·2914	+0·00036
„ (Ramleh)	d'Abbadie.	1884	0·2971	+0·00067
Cairo, Abbassia	d'Abbadie.	1884	0·30159	—
	H.G.L.	1895	0·30076	−0·00008
	H.G.L.	1896	0·30038	−0·00010
Great Pyramid	d'Abbadie.	1885	0·3034	—
	H.G.L.	1901	0·2992	−0·00026
Luxor	d'Abbadie.	1885	0·3249	—
	H.G.L.	1895	0·32070	−0·00042
Assuan	d'Abbadie.	1885	0·3259	—
	H.G.L.	1895	0·32364	−0·00023

As the observations for declination given in Tables I*a*, *b*, *c* and II*a* were all made between the autumn 1893 and the spring 1895, during which time the secular decrease would have been about 5′, equal to about half the amount of the diurnal variation, no reduction to an epoch has been attempted. The values obtained by observation have been plotted on the accompanying map, and the isogonic lines drawn between them by hand.


MEDITERRANEAN SEA
Damietta
Rosetta
Port-Said
ALEXANDRIA
Ismailia
Deir Aadi
Bishbishi
Deir Baramus
CAIRO
Pyramids
Saqqara
Helwan
Suez
GULF OF SUEZ
Maghagha
BAHARIA OASIS
Mandisha
Minia
Tur
FARAFRA OASIS
Assiut
Sohag
Girga
Kena
Luxor
Mut
DAKHLA OASIS
Kharga
Esna
Mualla
KHARGA OASIS
Edfu
Beris
KURKUR OASIS
1st CATARACT
Assuan
Dabod
Tropic of Cancer
Bir Murr
Abu Har
Gala
Amada
Sebua
Abu Simbel
Korosko
Shebb
Akshi
2nd CATARACT
Wadi-Halfa
Mirgissa
SELIMA OASIS
Wadi Magharin
Murrat
28° East of Greenwich
Isogonic Lines
Dip

These appear to show abnormally high values at Qena where the faulting of the Nile Valley is highly developed, and also at Esna and at Akshi (Serra) between Abu Simbel and Wadi Halfa. At this place, too, the horizontal force has a high value.

Appendix.

Observations on the Upper Nile.

The following observations were taken in March and April of the present year (1901) while accompanying Sir W. E. Garstin, K.C.M.G., Under-Secretary of State for Public Works, from Khartum through the region of the "Sadd" to Gondokoro and back, and are added here with his permission. As this journey was specially undertaken to see the upper reaches of the Bahr el Jebel, and to measure the discharge of the White Nile and its various tributaries in this district, magnetic observations had to be taken whenever opportunities occurred. It has consequently happened that the stations occupied cannot be described with sufficient accuracy for them to be re-occupied at a future date, since most of them were wooding-stations with no permanent building or other marks in the neighbourhood.

The latitudes given are taken from the map of the Bahr el Abiad (White Nile) made under the direction of General Gordon, when Governor-General of the Sudan, for the stations on that river; and those on the Bahr el Jebel from a compass survey of the river made on this occasion and adjusted to the latitudes of Gaba Shamba, Kenisa, Bor, Lado, and Gondokoro, which have been determined by observations at various times. (See map, p. 24.)

The station occupied at Omdurman was on the left bank of the Nile, half-way between the gunboat workshops and the angle of the old Omdurman wall, and about 100 metres from the river bank.

These observations, extending as they do from about 16° to 5° north latitude, and crossing the magnetic equator, form an interesting continuation to those from Cairo to Wadi Halfa, 30° N. to 21° 30′ N., which have been given above.

No attempt has been made to reduce these southern results to the same epoch as the others, since no reliable data are as yet available for doing so. Hardly any observations exist, it seems, which can be utilised to determine the secular change. Pruysenaere's results (quoted in the following short table taken from 'Petermann's Mittheilungen,' Ergänz.-heft 51, 1877) in the desert east of the Bahr el Abyad (White Nile) appear to show too much local attraction to be of much use.

An observation of Russegger's in April, 1837, at Torra, on the Bahr el Abyad, gives 9° W. for the declination, which, taking the present value at 5° 20′ W., gives 3° 4′ of annual decrease.

Place.	Lat. N.	Long. E.	Declination. 1863–64.
J. Abel	12° 42′	34° 25′	9° 10′
J. Qerebin	12 8	34 15	20 01
Werkat (southern part)	12 8	34 10	6 30
Roro	11 54	34 3	12 05
Gule	11 43	33 57	8 40

Also Captain A. W. Peel gives the West Declination at Khartum, as 8° 30′ in October, 1851, and Lieut. Watson, R.E., gives 7° 30′ West, for that of Rejaf (lat. 4° 40′ ±), on 15th December, 1874.

The only other observation it has been possible to find in the books of travel, &c., available in Cairo, is a value of 7° 30′ W. for the declination at Gondokoro on March 20, 1861,* which with 6° 20′ for the present values gives − 2′ per annum.

Unfortunately the first rains were already threatening at Lado and Gondokoro, and the sky was usually too cloudy to admit of satisfactory observations for azimuth, Hellet el Nuer and El Kenisa were therefore the only two places where the declination could be determined on the Bahr el Jebel. Russegger also gives for El Obeid declination 8° 30′ west and dip 18° for April, 1837, but neither of these can be utilised, being too far from the Nile.

The instruments used were Kew-pattern Magnetometer, No. 87, by Elliott, and a Dip Circle, No. 131, by Dover.

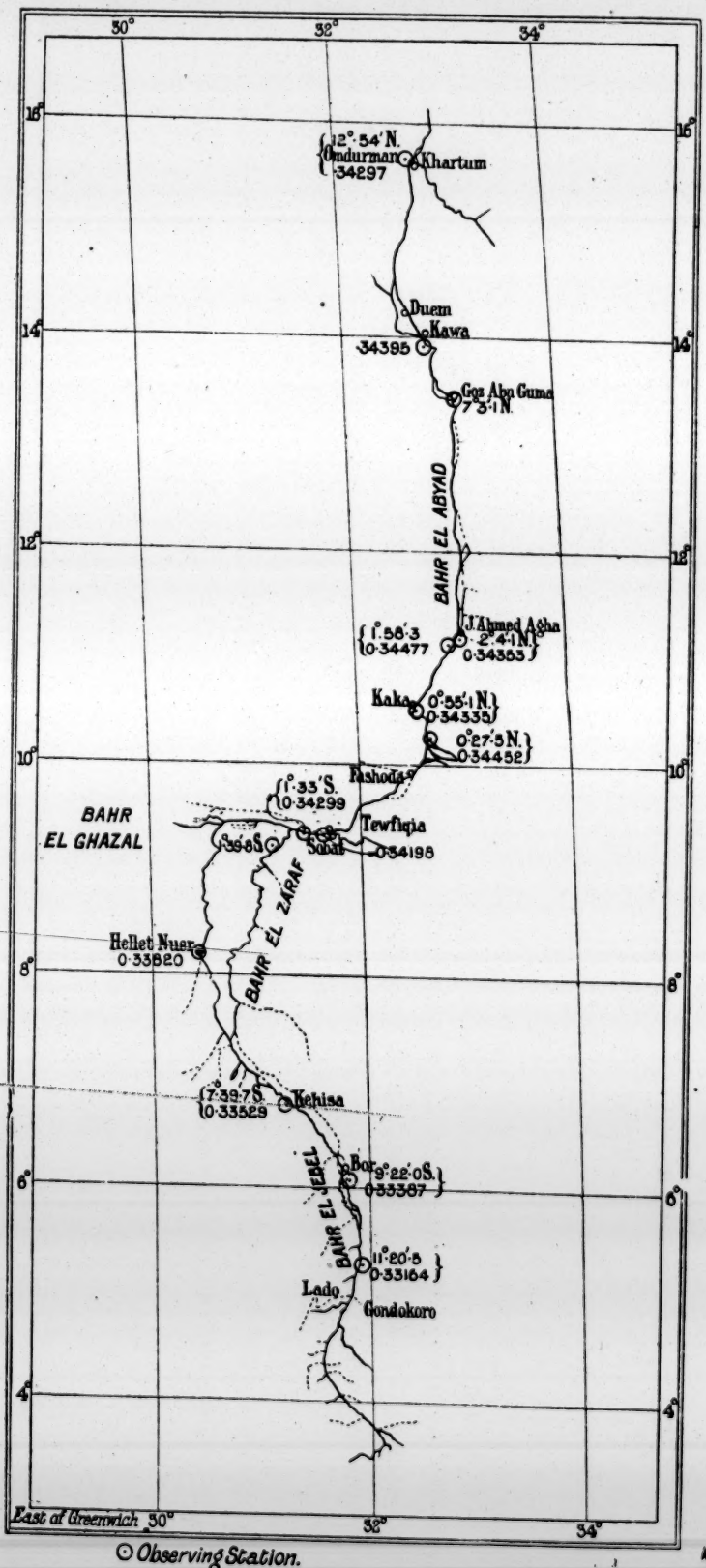
* Peney, 'Bull. Soc. Géog. Paris,' 1863.

Observations of Dip and Horizontal Force on Bahr el Abyad and the Bahr el Jebel.

Place.	Date.	Mean time 30° E. of Greenwich.	Dip.	Mean time. 30° E.	Horizontal force.	
		h. m.	° ′			
Omdurman, left bank of Nile	12–4–1901	16 25	12 54·4 N.	—	0·34297	
Kawa, left bank of Nile	10–4–1901	—	—	—	0·34395	
Goz Abu Goma, right bank	10–4–1901	8 25	7 3·1 N.	—	—	
Near J. Ahmed Agha, right bank ..	8–4–1901	8 15	2 4·1 N.	—	0·34353	
„ „ „ left „ ..	14–3–1901	11 40	1 58·3 N.	—	0·344774	
„ Kaka	7–4–1901	12 20	0 55·1 N.	—	0·34335	
North of Fashoda, right bank.....	15–3–1901	10 15	0 29·2 N. 0 25·3 N.	—	0·34452	
S.W. of Sobat mouth „ „	16–3–1901	—	—	—	0·34198	
E. of Bahr el Zaraf mouth, right bank	4–4–1901	12 0 14 30	1 31·6 S. 1 32·6 S. 1 34·9 S.	—	0·34299	1° 34′·9 obtained from obs. 10° E. and W. of meridian.
Bahr el Zaraf, left bank	3–4–1901	17 20	1 39·8 S.	—	—	
Hellet el Nuer „ „	20–3–1901	—	—	—	0·33820	No deflection obs.
Kenisa, left bank	23–3–1901	17 15	7 42·0 S. 7 37·4 S.	—	0·33529	
„ „ „	31–3–1901	—	—	—	0·33506	
S. of Bor, right bank	25–3–1901	12 15	9 22 0 S.	—	0·33387	No deflection obs.
S. of Kiro „ „	26–3–1901	17 20	11 20·5 S.	—	0·33164	

Magnetic Declination determined on White Nile and Bahr el Jebel with Kew-pattern Magnetometer No. 87.

Place.	Date.	Latitude, N.	Mean time 30° E.	Declination west.	
		° ′ ″	h. m.	° ′	
Omdurman, near steamer workshops	12-4-1901	15 39 40	17 28	4 39·5	
Renk	13-3-1901	12 0 0	8 0	6 4·5	Probably local disturbance.
East bank, 6 kiloms. south of Jebel Ahmed Agha	8-4-1901	11 1 0	6 45	5 7·1	
West bank, south of Jebel Ahmed Agha	14-3-1901	10 58 0	9 20	4 57·6	
Wood station, near Kaka, right bank	17-4-1901	10 30 0	14 5	4 55·7	
Wood station, right bank, 48 kiloms. north of Fashoda	15-3-1901	10 20 0	8 30	5 8·9	
Wood station, right bank, 8 kiloms. south of mouth of R. Sobat	16-3-1901	9 22 0	16 10	5 43·7	
Left bank of Bahr el Jebel, 10 kiloms. south of Hellet Nuer	20-3-1901	8 9 30	17 15	5 49·6	
Kenisa, left bank of Bahr el Jebel	23-3-1901	6 46 0	16 20	6 13 8	


30°
32°
34°
16°
14°
12°
10°
8°
6°
4°
12°·54′N.
Omdurman
·34297
Khartum
Duem
Kawa
34395
Gog Abu Guma
7°·1N.
BAHR EL ABYAD
1°·56′3
0·34477
J. Ahmed Agha
2°·4·1N.
0·34383
Kaka
0°·55′·1N.
0·34335
0°·27′·5N.
0·34452
Fashoda
1°·33′S.
0·34299
Tewfiqia
0·34198
BAHR EL GHAZAL
BAHR EL ZARAF
Hellet Nuer
0·33820
7°·39′7S
0·33529
Kchisa
Bor
9°·22′·0S.
0·33387
BAHR EL JEBEL
11°·20′·5
0·33164
Lado
Gondokoro
East of Greenwich
⊙ Observing Station.

Description of Stations.

Place.	Bank of river.	Observations made.
Omdurman, 200 metres south of steamer workshops	Left	Declination. Dip. Horizontal force.
Kawa (lat. 13° 55′)	„	Horizontal force.
Goz Abu Goma (lat. 13° 23′)	Right	Dip.
Renk	„	Declination.
6 kilometres up stream of Jebel Ahmed Agha	„	Declination. Dip. Horizontal force.
16 „ „ „ „	Left	„ „ „
Near Kaka (lat. 10° 30′)	Right	„ „ „
48 kilometres down stream of Fashoda (lat. 10° 20′)	„	„ „ „
8 kilometres up stream of mouth of River Sobat, on Bahr el Abyad	„	Declination. Horizontal force.
Wood station on Bahr el Abyad, east of mouth of Bahr el Zaraf 12 kilometres	„	Dip. Horizontal force.
Bahr el Zaraf, 20 kilometres from mouth	Left	Dip.
Bahr el Jebel, 10 kilometres	„	Declination. Horizontal force.
South of Hellet Nuer (lat. 8° 9′ 30″)		
Kenisa (lat. 6° 46′ 0″)	„	Declination. Dip. Horizontal force.
Wood station, south of Bor on Bahr el Jebel (lat. 6° 5′)	Right	Dip. Horizontal force.
Wood station, south of Kiro on Bahr el Jebel (lat. 5° 15′)	„	„ „

"Note on the Effect of Mercury Vapour on the Spectrum of Helium." By Professor J. NORMAN COLLIE, F.R.S. Received June 3,—Read June 19, 1902.

Some years ago the author, in conjunction with Professor Ramsay, published the results of some experiments relating to the visibility of the spectrum of one gas in presence of another.*

Since then some experiments have been made on the effect of mercury vapour (when present in considerable quantity) on the spectrum of helium in an ordinary Plücker's tube, under the influence of the electric discharge from an induction coil. When the spectrum of helium is examined in an ordinary Plücker's tube, it appears to be a simple one consisting of only eight lines—two red, one yellow, three green, one blue, and one violet.

The spectrum, however, of the negative glow is much more com-

* 'Roy. Soc. Proc.,' vol. 59, p. 262.

A2546

plicated, a large number of other and fainter lines appearing as well. If mercury vapour be introduced into such a tube filled with helium at from 2—5 mm. pressure, the first change noticed is the simplification of the spectrum of this negative glow; and at the same time three of the helium lines disappear from the spectrum of the negative glow, the red 7065, the blue 4713, and the violet 4472, whilst the yellow 5876 becomes very feeble. In the orange part of the spectrum a brilliant new line can now be seen, namely, the mercury line 6151, a line which as a rule does not appear in a Plücker's tube containing mercury vapour when an electric discharge from an induction coil is passed through it. The presence of the orange line of mercury is of interest, as other gases, argon, krypton, hydrogen, &c., mixed with mercury vapour, when examined under the same conditions fail to give it.

Whether this orange line is shown in a neon tube where mercury vapour is present is difficult to say, as one of the bright orange lines of neon almost coincides with it.*

Besides this alteration of the helium spectrum at the negative electrode another curious change can be observed, if a piece of tubing (whose internal diameter is about 4 mm.) be introduced into the centre portion of the Plücker's tube. The spectrum of the gas in this central portion invariably consists of the mercury lines in the yellow, green, and violet, together with one and one only of the helium lines, namely, the green line 4922. This seems to be a most delicate test for minute traces of helium in other gases, and this line can be easily seen when no other helium lines can be detected elsewhere in the tube. A large excess of mercury vapour will also produce the same result in the spectrum of the glowing vapour in the capillary bore tube.

Thus in the same Plücker's tube, containing helium and mercury vapour, helium may be made to yield three distinct spectra:—

(1.) In the narrow bore capillary portion the full spectrum of eight lines.

(2.) At the negative electrode, three lines disappear, one red, one blue, one violet, and the yellow line becomes very faint.

(3.) In the wider bore central portion only one green line is visible.

Of course the mercury spectrum is visible in all portions, but at the negative electrode the orange mercury line becomes the most brilliant.

Another interesting fact is the great purity of the spectrum. In every case there is a marked absence of light between the lines.

Runge and Paschen pointed out in 1895† that the helium spectrum seemed to belong to two systems.

The lines which persist and those which disappear in the negative

* The author has to thank Professor Ramsay for kindly helping him and for supplying the rare gases neon and krypton for these experiments.

† 'Phil. Mag.,' vol. 40, p. 297.

glow coincide with these two systems. For all those lines which disappear in the negative glow, 7065, 4713, 4472, and the yellow line 5876 which nearly does so, belong to that system which, according to Runge and Paschen, is due to the gas helium; whilst these which can be seen at the negative glow with their full brilliance, 6677, 5016, and 4922, belong to the second system.

This differentiation of the helium spectrum brought about by the presence of mercury vapour might at first sight appear as a confirmation of the idea of Runge and Paschen that helium is a mixed gas, consisting of two different elements. But taken in conjunction with the fact that the spectra of argon, neon, and krypton are all altered by the same treatment, no reliance necessarily can be placed on the argument.

In conclusion it is worth while pointing out that a helium-mercury tube containing the merest trace of hydrogen should be of value as a standard tube for spectroscopic measurements. For it contains fourteen standard lines, amongst them (C and F). Moreover all these lines are of brilliance ten, very equally spaced from the extreme red to the violet, and with dark spaces between them.

He	Red	7065	Hg	Green	5461
He	Red	6677	He	Green	5016
H	Red	6563 C	He	Green	4922
Hg	Orange	6151	H	Green	4861 F
He	Yellow	5876	He	Blue	4713
Hg	Yellow	5790	He	Violet	4472
Hg	Yellow	5769	Hg	Violet	4359

"The Seed-fungus of *Lolium temulentum*, L., the Darnel." By E. M. Freeman, M.S., University of Minnesota. Communicated by Professor Marshall Ward, F.R.S. Received June 6—Read June 19, 1902.

(Abstract.)

Darnel (*Lolium temulentum*, L.) has been known since Roman times for the poisonous properties of its grain. It was not, however, until 1898 that the presence of an often considerable layer of hyphæ was discovered just exterior to the aleurone layer of the grain; to the action of this fungus-layer the poisonous properties are presumably due.* Nothing had hitherto been known regarding the method of

* How far ergot and other fungi may be concerned is a disputed point.—[June 24.]

infection of the plant, although the presence of hyphæ in the growing point from about the 8th day of germination to the formation of the fruit had been observed by one investigator, and but little positive evidence as to the real nature of the fungus has been produced.

In addition to the now known hyphal layer outside the aleurone of the grain the author has discovered a hitherto unnoticed patch of hyphæ, just outside of and contiguous to the base of the scutellum. From this patch hyphæ can be found penetrating to the growing point of the embryo in the seed, and an abundant mycelium can usually be detected in the young growing point. The hyphæ continue their growth, always intercellular, in the growing point and in the leaf-bases until the formation of the inflorescence, when every young ovule contains an abundance of hyphæ throughout the nucellus, and extending to within two cells of its exterior. When the ovule elongates and assumes the ovoid form, there is formed a tongue of hyphæ extending in the nucellus, from the funicular side to the micropyle on the inner or axial side of the embryo-sac. This patch becomes isolated by the further elongation of the ovule, and by the cessation of growth of the hyphæ in the funicle, and remains vigorous, forming an *infection layer*. When the embryo has attained a length of about 3/10 mm. and the rudiments of a lateral growing point and a terminal scutellum have been formed, the hyphæ penetrate from the *infection layer* into the growing point, and can be found here in abundance in the mature grain.

No trace of any formation of spores can be found anywhere, and they seem therefore unnecessary for the ordinary life-cycle of the fungus. The bulk of the nucellar hyphæ of the ovule are crowded into the layer occupying the now well-known position in the grain, by the growth of the endosperm. They sometimes penetrate the latter, but have never been seen to form either sclerotia or spores. This layer disintegrates during the germination of the grain.

Other varieties or species of *Lolium* also contain such a hyphal layer occasionally, probably identical with that of Darnel. I have found it in *L. temulentum*, var. *arvense*, With., and *L. linicola*, Br., very abundantly, and rarely in *L. perenne*, L., *L. multiflorum*, Lam., and *L. italicum*, Br., besides in several doubtful species. Several facts—prominent among which are the ordinary sterility of the fungus and the apparent stimulation of the Darnel plant, as shown in the germination efficiency and the large size of the grains—point strongly to this being a remarkable instance of symbiosis. Such a symbiosis can be conceived as derived through a sclerotial or spore-forming condition by the substitution of an intra-seminal infection of the embryo after failure to reproduce in the usual way.

The principal points dealt with in the full paper, which is illustrated by figures, may be shortly summarised as follows :—

Summary.

1. The proportion of grains without to those with the fungus layer outside of the aleurone of *L. temulentum* varies, but is usually from 80—100 per cent. In some cases the microscopic differences are sufficient for diagnostic purposes.

2. The hyphæ sometimes penetrate the aleurone at any point and invade the starch-endosperm, but no fructification or spores of any kind have been seen.

3. There exists in the nucellus, at the base of the scutellum and at the lower end of the inner groove of the grain, a layer of hyphæ which lies directly against the embryo, constituting an *infection layer*.

4. Embryos of the grain of *L. temulentum* in a proportion approximately equal to that of the occurrence of the fungus in the grains, always contain hyphæ in the growing point, and these hyphæ can be traced to their origin in the *infection layer*. They remain in the growing point throughout the life of the plant.

5. The course of the hyphæ is always intercellular.

6. There is no reason for the supposition of any so-called "mycoplasm" in the embryo: the fungus always exists as distinct hyphæ.

7. In the growing plant the fungus forms networks in the leaf-bases, of which the function has not yet been determined.

8. The nucellar hyphæ of the grain undergo degeneration during germination without the formation of spores.

9. All attempts to obtain cultures of the nucellar hyphæ have failed, indicating either that the hyphæ here have lost their vitality or else, as is more probable, that they are too closely adapted to a parasitic (or symbiotic) life to allow of artificial culture.

10. In the young ovary a tongue of hyphæ reaching from the funicle to the micropyle on the inner side of the grain, becomes detached from the remaining nucellar hyphæ by the elongation of the ovule and the cessation of hyphal growth in the funicle.

11. Infection takes place from this layer as soon as the growing point of the embryo has appeared as a recognisable rudiment.

12. The fungus of other species seems very probably identical with that of *L. temulentum*, as cross-infections seem to be possible by grafting. *L. multiflorum*, Lam., and *L. italicum*, Braun., also contain a hyphal layer.

13. There are serious objections to the reference of this fungus to the Ustilagineæ, to the ergot-forming Pyrenomycetes or the Hyphomycetes (such as Woronin and Prillieux and Delacroix have described for poisonous rye), or to the suggestion that it is a Uredine.

14. The effect of the fungus upon its host seems rather beneficial than detrimental, and several facts point toward the confirmation of

Guerin's suggestion of a symbiotic relationship. Such can be conceived of as arising from a former sclerotium- (or spore-) forming habit by the adoption of a new intra-seminal mode of infection.

"On the Effects of Magnetisation on the Electric Conductivity of Iron and Nickel." By GUY BARLOW, B.Sc., Research Fellow of the University of Wales. Communicated by Professor A. GRAY, F.R.S. Received February 20,—Read March 6,—Received in revised form June 18, 1902.

That magnetisation has an effect on the electric conductivity of metals was first noticed in 1856 by William Thomson* (Lord Kelvin), and since then, on account of its very great theoretical interest, this phenomenon has formed the subject of numerous experiments. These later investigations have been made by Tomlinson,† Righi,‡ Hurion,§ Leduc,‖ Ettingshausen and Nernst,¶ Ettingshausen,** Goldhammer,†† Lenard and Howard,‡‡ Lenard,§§ Henderson,‖‖ Beattie,¶¶ Gray and Jones,*** and others.

Two cases of the phenomenon must be carefully distinguished—(i.) Longitudinal effect, when the direction of the electric current is parallel to the lines of magnetic force. (ii.) Transverse effect, when the current is perpendicular to the lines of force. The latter effect is closely connected with the Hall phenomenon, and has received the name of Longitudinal Hall Effect.

The following results obtained by previous experimenters are of interest in connection with the present investigation. For bismuth, Goldhammer found the change of resistance to vary as the square of the magnetic field, and since in this metal the magnetisation I is proportional to the field, this result may be written $\Delta\phi = aI^2$, where $\Delta\phi$ denotes the *fractional* increase of resistance, *i.e.*, the increase of resist-

* 'Math. and Phys. Papers,' vol. 2, p. 307.
† 'Phil. Trans.,' 1883.
‡ 'Journ. de Physique,' vol. 3, p. 355 (1884).
§ 'Compt. Rend.,' vol. 98, p. 1257 (1884).
‖ 'Compt. Rend.,' vol. 98, p. 673 (1884).
¶ 'Wien. Ber.,' vol. 94, part 2, p. 560 (1886).
** 'Wien. Ber.,' vol. 95, p. 714 (1887).
†† 'Wied. Ann.,' vol. 31, p. 360 (1887); vol. 36, p. 804.
‡‡ 'Electro-technische Zeitschrift,' vol. 9, p. 341.
§§ 'Wied. Ann.,' vol. 39, p. 619 (1890).
‖‖ 'Phil. Mag.,' vol. 2, p. 488 (1894).
¶¶ 'Phil. Mag.,' vol. 1, p. 243 (1898).
*** 'Roy. Soc. Proc.,' 1900, vol. 67.

ance divided by the resistance in zero field; and a is a constant. It is therefore suggested by Goldhammer that the above relation may be also true for the ferro-magnetic metals. In order to verify this a direct determination of the magnetisation is necessary. Beattie has investigated the transverse effect in thin films of iron, nickel, and cobalt, and determined the magnetisation by means of Kundt's result that for such films the Hall effect and the magnetisation are proportional. The relation $\Delta\phi = aI^2$ was confirmed in the case of the cobalt films, but not in those of nickel. In iron the variation of resistance was so small that accurate results were not obtained. In the more recent investigation of Gray and Jones on the change of resistance of iron wire magnetised longitudinally, the relation $\Delta\phi = aI^4$ was suggested as approximately representing their results between the field strengths 30 and 250 c.g.s.

The present investigation had for its object a more exact determination of the relation between the change of resistance and the magnetisation in the two ferro-magnetic metals—iron and nickel. As in the experiments of Gray and Jones the metal was in the form of a wire, so that a close approximation to uniform longitudinal magnetisation could be obtained. In the course of the investigation the hysteresis of resistance was found to be of such importance that this phenomenon was separately examined. Some determinations of the transverse effect were also included.

Experiments in Ordinary Magnetic Fields.

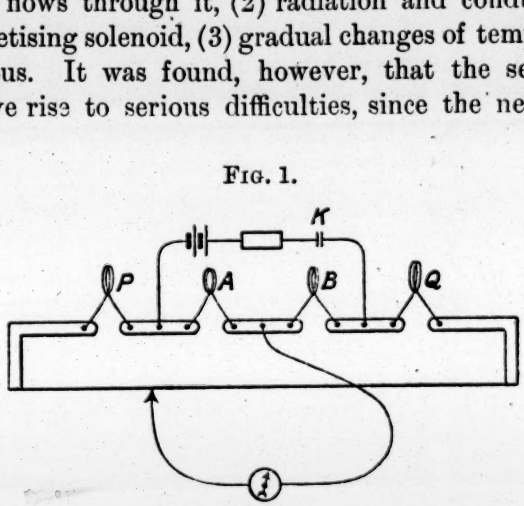
These experiments were made with three different specimens of wire, iron, steel, and nickel.

The magnetic field was obtained by means of a solenoid 1 metre in length, which was placed vertically. The current for this solenoid was produced by a battery of storage cells, and measured by means of a Kelvin graded galvanometer which was standardised from time to time. The maximum field was 450 c.g.s.

The Wheatstone-bridge method was used to determine the change of resistance, the connections being shown in fig. 1. P is the iron or nickel wire under investigation, Q a *comparison coil* of equal resistance. A very thick but uniform wire of German-silver was used for the bridge, but for measuring very small effects this was replaced by a nickel-plated copper wire having a much smaller resistance per unit length. A and B are two equal *auxiliary coils* of German-silver wire, immersed in an oil bath to avoid temperature differences. A Kelvin astatic galvanometer G of resistance 3·6 ohms was used in these experiments. In some preliminary measurements on the resistance change in the iron wire, the comparison resistance Q consisted of a spiral coil of the same wire. This method was first used by Gray

and Jones, and theoretically it should eliminate the temperature effects due to (1) the Joule heating effect in the iron wire caused by the current which flows through it, (2) radiation and conduction of heat from the magnetising solenoid, (3) gradual changes of temperature in the whole apparatus. It was found, however, that the second of these effects still gave rise to serious difficulties, since the necessary insula-

FIG. 1.



tion between the coils P and Q prevented rapid equalisation of temperature. Thus the transmission of heat from the solenoid affected the spiral coil Q sooner than it affected the coil P, and hence the equilibrium of the bridge was disturbed. This method was therefore modified, the following arrangement being finally adopted. It will be sufficient to describe the experiments on nickel, as in the case of the other two metals apparatus was essentially the same.

The resistance P consisted of two complete turns (*i.e.*, four lengths) of the nickel wire wound longitudinally on a thin rod of wood 65 cm. long. The connections to the bridge were made of thick copper wire soldered to the ends of the nickel coil. The resistance Q was equal to P, but consisted of thin copper wire wound in a similar way on the same rod. All the wires had double silk insulation. The coils were then covered with two layers of white wool and placed in a long glass tube which fitted into the magnetising solenoid; also the latter was provided with an internal water jacket, through which flowed a stream of cold water from the mains, the temperature of the water on entering and leaving the solenoid being indicated by two thermometers inserted in capsules through which the water flowed. This arrangement prevented rapid transmission of heat to the coils P and Q, so that the heating effect was always small, and varied very slowly; moreover, the heating effect in Q sufficiently compensated that in P. The current in the nickel wire never exceeded 0·05 ampere, and the bridge circuit being only closed for very short intervals, this current caused no observable change of resistance through the Joule heating effect.

The changes of resistance are determined as follows:—The nickel is first thoroughly demagnetised by the process of reversals; the reading x_0 of the bridge is then taken as soon as the equilibrium becomes steady. The magnetising circuit is then closed, and after reversing the current a number of times to ensure reaching a point on the curve of ascending reversals, the new reading x is taken. Finally, the magnetising current is broken, and the residual reading x' determined. Then $\Delta x = x - x_0$ is the "step" on the bridge wire due to the induced magnetisation, and $\Delta x_r = x' - x_0$ is the corresponding residual step. In taking the readings, the sliding contact piece is moved along the bridge until the point is reached at which no *permanent* deflection of the galvanometer needle takes place when the bridge current is made or broken. This was found to be the most satisfactory way of eliminating the thermo-electric effects in the bridge wire. Before each set of readings the specimen was reduced to a neutral state by the process of demagnetisation, thus avoiding the errors due to slow changes of temperature.

The fractional change of resistance $\Delta\phi$ in the nickel wire P corresponding to the step Δx is calculated from the formula

$$\Delta\phi = \frac{\Delta P}{P} = \frac{A+B}{A.B}.\sigma\Delta x,$$

where σ is the resistance per unit length of the bridge wire. A slight correction is required for the resistance of the copper leads connecting P to the bridge.

A preliminary experiment was made to test the effect of the magnetic field on the resistance of the copper comparison coil Q. The nickel coil was disconnected from the bridge, an equal resistance of German-silver being substituted. Only the copper coil Q remained in the solenoid, and as no change of resistance was observed, it was concluded that in copper the effect is negligible.

The magnetisation curves were obtained by the ballistic method. Seven pieces of the nickel wire, each 65 cm. long, were enclosed in a thin glass tube, round the middle of which was wound a ballistic coil of 200 turns of fine copper wire. The glass tube was placed in the axis of the magnetising solenoid, and the coil connected to a ballistic galvanometer provided with telescope and scale. The galvanometer was standardised before and after each set of readings by means of a standard solenoid and secondary coil.

The true magnetic force H in the nickel is given exactly by $H = H' - NI$ where H' is the magnetic field, calculated from the known constant of the solenoid and the strength of the magnetising current, and N is the demagnetising factor for the bundle of wire. In both the resistance and magnetic apparatus the value of N was less than 0·0005, so that this term could be neglected.

Results.—The corresponding values of $\Delta\phi$, H, and I for the process of ascending reversals of magnetisation in nickel, iron, and steel are contained in Table II. The change of resistance is always an increase, and is about eight times greater in nickel than in iron or steel for the same field. If we take $\Delta\phi$ as ordinate and H as abscissa, the curves obtained will be found to have the same characteristics as the ordinary magnetisation curves, except that the points of inflection are not so strongly marked. The result is quite different if I be taken as abscissa. In this case we obtain for the three metals a curve which has no point of inflection, but which resembles in general form a semicubical parabola, the curve being remarkably flat near the origin and then rising very steeply. Figs. 2 and 3 show the relation of $\Delta\phi$ to I for nickel and iron respectively, the abscissa being the *square* of the magnetisation. The curve for steel differs very little from that of iron.

For the three metals it is found that the relation $\Delta\phi = aI^4$, suggested by the experiments of Gray and Jones, is not generally satisfied (*i.e.*, the curves in figs. 2 and 3 are not parabolas), although it is nearly true for strong fields in the case of iron. In weak fields the variation of resistance appears to depend on a lower power of the magnetisation, and in order to represent the results for the three metals by the same formula it was found necessary to adopt the more general expression

$$\Delta\phi = aI^2 + bI^4 + cI^6,$$

which must only be regarded as a convenient method of expressing the results. The relative values of the coefficients a, b, and c, are as follows:—

Nickel.....	$a : b : c$	$= 19 : 57 : -4$
Iron	$a : b : c$	$= 74 : 3 : 31$

Thus the term containing I^4 is relatively much more important in nickel than in iron.

An examination of the hysteresis effects in the three metals led to some interesting results which will be best understood by reference to fig. 4. This curve exhibits the variation of resistance in the nickel wire for a cycle of magnetisation corresponding to a double reversal of a magnetic field of about 165 c.g.s. The resistance hysteresis loops for the three metals possess the same characteristic features. As the field diminishes from its maximum value, H = +165 to zero, the descending branch of the curve lies above the ascending, and in zero field the two branches intersect at the point which represents the residual change of resistance. Then as the field is reversed to small negative values, the resistance continues to diminish until at a certain critical value of the field the curve reaches a minimum. After passing this point the curve rises rather steeply (especially in iron and steel),

FIG. 2.

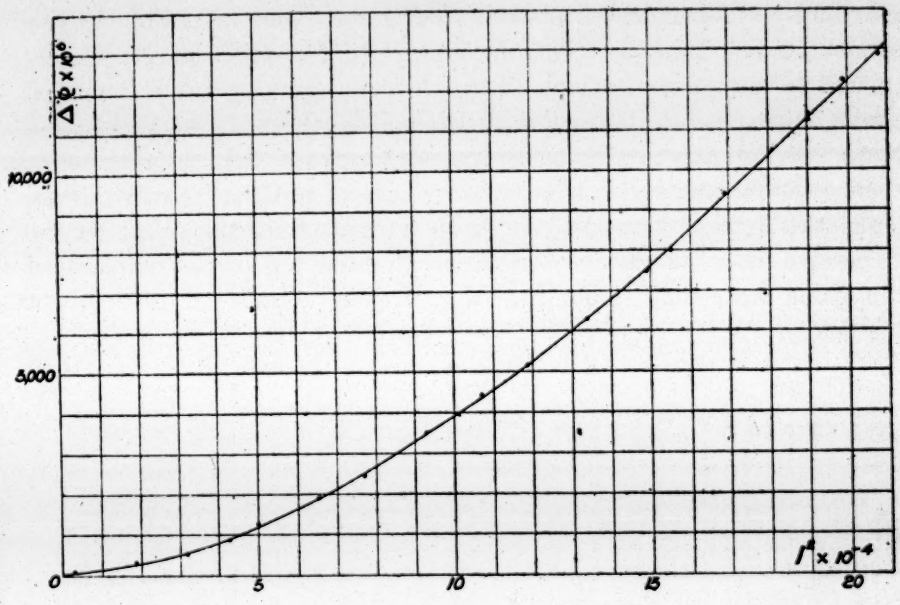
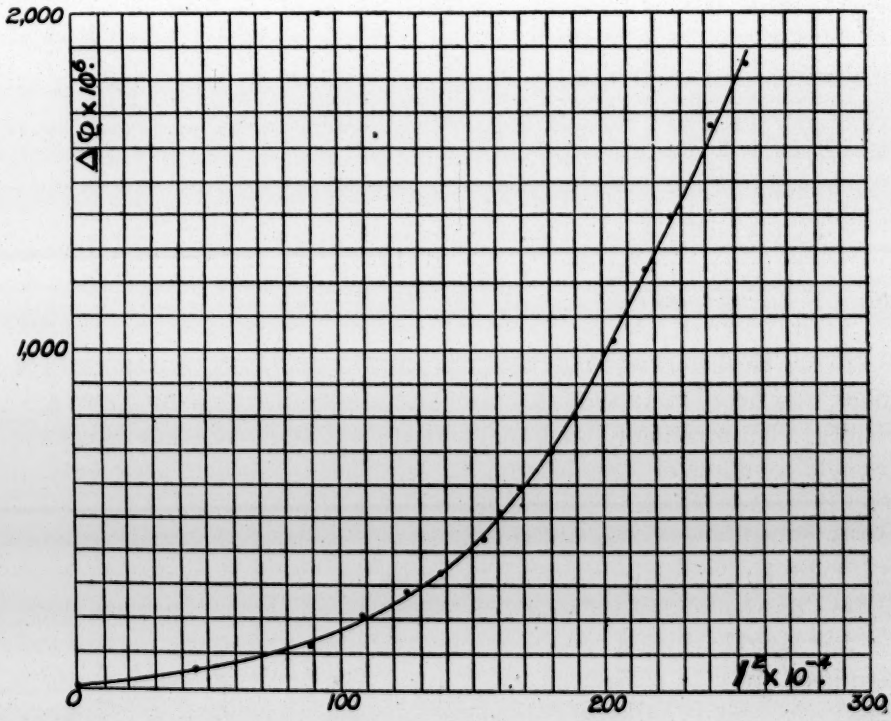


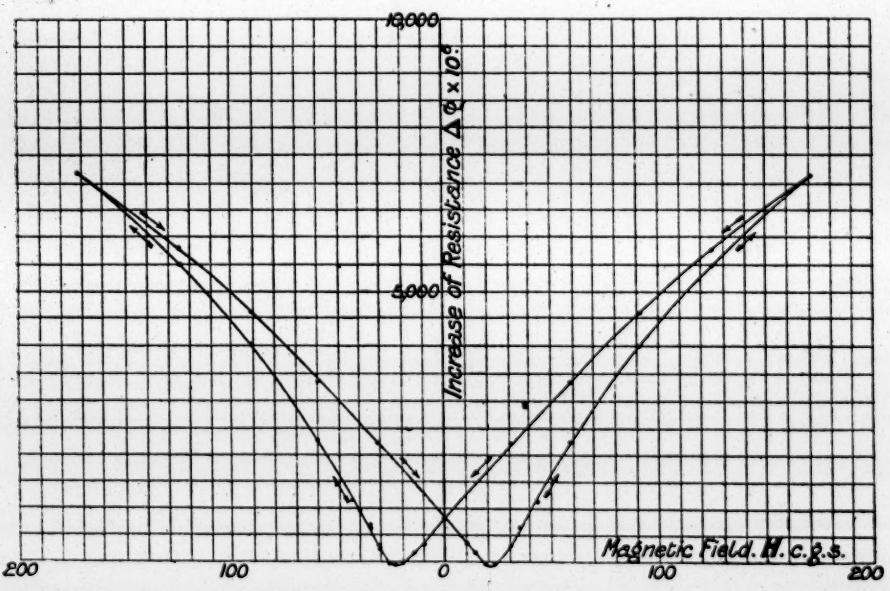
FIG. 3.



and, after inflection, attains its highest point with the maximum field H = −165. The loop is completed by a similar curve as the field changes from −165 to +165 units. At the two minimum points the

curve appears to *touch* the axis. The magnetic hysteresis loops were also obtained for exactly the same cycle, and a comparison of the two sets of curves indicates that the change of resistance is very closely related to the magnetisation. Thus the "minimum points," described above, appear to be identical with the points where the magnetisation loops *cut* the axis; or in other words, when the field is such as to reduce the magnetisation to zero, the change of resistance also vanishes. Thus this critical value of the field is approximately equal to the "coercive force" of the specimen. In all cases the curves represented the cyclic state that is obtained after a large number of reversals of the field.

FIG. 4.



It may be remarked that the corresponding $\Delta\phi$, I curves also possess loops, and hence it appears impossible to express $\Delta\phi$ as a function of I simply. Unfortunately the magnetisation loops were not accurate enough to allow of any attempt to establish a relation between the quantities $\Delta\phi$, H, and I for the general case of hysteresis.

For nickel it was observed that if at any point of the cyclic process the field be suddenly reduced to zero, the residual resistance will depend on the particular point of the cycle which has been reached. In the neighbourhood of the minimum points reducing the field to zero may cause an *increase* of resistance. The minimum residual effect was only about half that corresponding to the fields $\pm$ 165 c.g.s. In the other two metals a similar effect was noticed, but the investigation was not carried further, owing to the difficulty of observing such very small changes of resistance. These effects cannot be explained as resulting from the action of the earth's field.

It should be mentioned that the change of length produced by magnetisation in iron and nickel is too small in moderate fields to account for the observed variation of resistance; it is, however, possible that this effect may give rise to an important correction in strong fields.

It is obvious that the method employed in these experiments completely eliminates any change of resistance which is reversed in sign by reversal of the field; but such an effect has never been observed.

Experiments in High Fields.

Experiment I.—Longitudinal Effect in Nickel.

Another specimen of wire was used having a diameter of 0·33 mm., and covered with double silk insulation.

It being desirable to determine the magnetic field and magnetisation under exactly the same conditions as the change of resistance, the apparatus was arranged so as to allow the three measurements to be made with the same nickel coil. A modification of the "isthmus" method was adopted. The magnetic field was produced by means of a large electromagnet provided with conical pole pieces having faces 1·7 cm. in diameter.

The essential part of the apparatus is represented diagrammatically in figs. 5 and 6. The nickel wire was wound in a single layer, in and out the thin glass cylinder C, 1·1 cm. long and 1·1 cm. in diameter, so that the turns of the coil, 88 in number, were *parallel* to the axis of the cylinder, as shown in fig. 5. The glass being thin, the *transverse* end elements of the coil were small in comparison with the length of the cylinder. There were three ballistic coils; the innermost was wound on a glass tube D, the second on the brass cylinder B, and the outermost on the brass flange A, which extended over half the cylinder B. This system of coils was attached to one end of a wooden arm, having a pivot at the other extremity, by means of which the coils could be suddenly placed in the proper position between the poles of the magnet. Before every reading the nickel was demagnetised by a separate arrangement. The ballistic deflections as well as the resistance change were produced by placing the coils in the field, the effects of residual induction in the nickel being thus avoided. There being no compensation for temperature variations, it was necessary to make the observations as quickly as possible. It was arranged that the ballistic deflections measured the differences of the total induction through the consecutive coils. The values of H and I were then easily calculated from the two observed deflections.

As usual, it is assumed that the value of the field just outside the nickel is the same as that in the metal. The innermost ballistic coil

merely served to eliminate the value of the field near the axis. This method of determining I becomes insensitive when H is very great; it was therefore found impossible to obtain measurements of the magnetisation in this case. It is probable, however, that the saturation stage of magnetisation was practically reached within the range of the experiment.

FIG. 5.

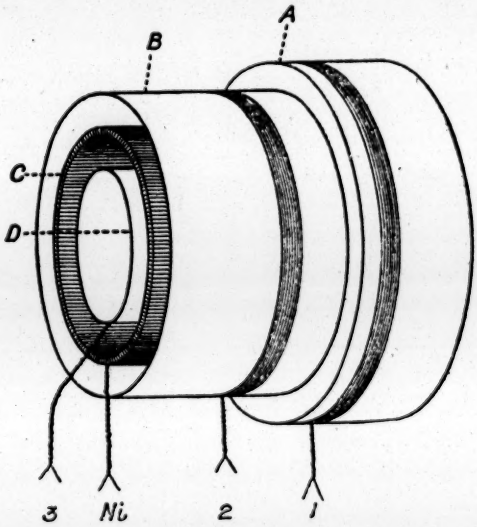
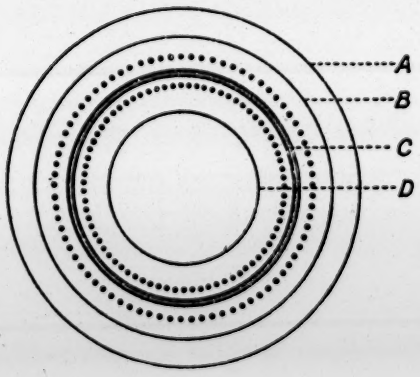


FIG. 6.



Conclusions.—That for fields ranging from 1000 to 11,000 c.g.s., the value of $\Delta\phi$ for longitudinal magnetisation in nickel wire is practically *constant*. This constant value

$$\Delta\phi = 0{\cdot}017$$

is greater than the values obtained in the experiments in ordinary fields.

The results indicated, however, that a slight *decrease* in $\Delta\phi$ takes

place in the highest fields, a peculiarity that could not be accounted for by the experimental errors. In order to investigate this effect in still higher fields, the following experiment was made :—

Experiment II.—Longitudinal Effect in Nickel.

The method used was the same as in Experiment I, but the apparatus was simplified and made as small as possible. The nickel wire was wound on a glass cylinder as before, the number of complete turns being thirty-four and length of cylinder only 5 mm. A small ballistic coil was placed in the axis of the nickel coil in order to determine the field. Finer pole pieces were used and the distance between them reduced to 7·5 mm. No attempt was made to measure the magnetisation, and the field determined by the ballistic coil is assumed to be the same as that in the nickel : as this had been proved to be approximately true in the former experiment.

Results.—The change of resistance now exhibits a decided maximum—

$$\Delta\phi = 0{\cdot}0156, \qquad H = 2000 \text{ c.g.s.},$$

and in higher fields decreases continuously to the value

$$\Delta\phi = 0{\cdot}0100, \qquad H = 18{,}000 \text{ c.g.s.}$$

To explain this result it appears necessary to consider the effect of the end-elements of the nickel coil. In this apparatus the end-elements formed a considerable fraction of the whole coil, whereas in Experiment I this fraction was small. These elements of the wire are magnetised *transversely*. Even if there were no transverse effect in nickel, the existence of the end elements reduces the observed change of resistance, and the necessary correction cannot be estimated. But the electrical resistance of nickel is diminished by the transverse magnetisation, and this effect may therefore easily explain the peculiar results of the above experiment.

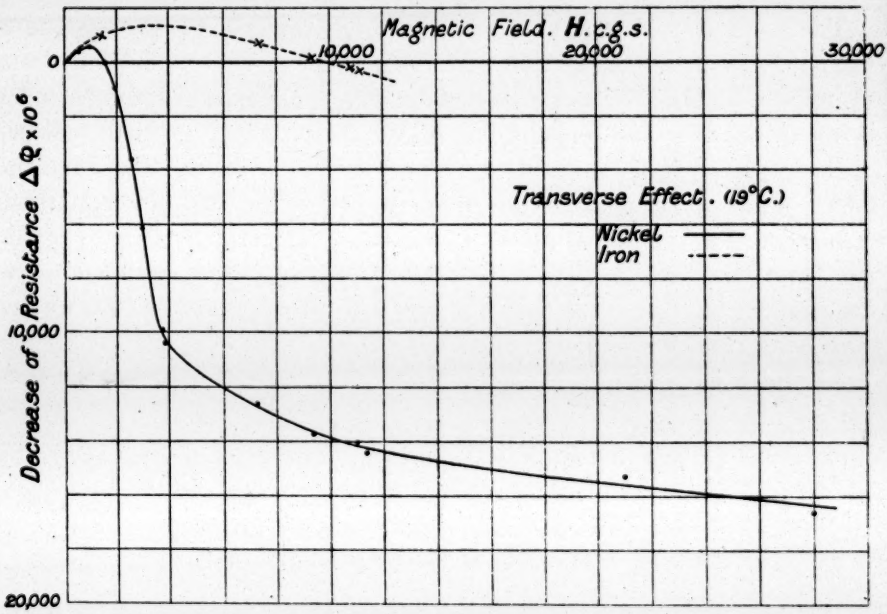
Experiment III.—Transverse Effect in Nickel.

The above experiments suggested that an examination of the transverse effect in the same specimen of nickel would prove of considerable interest. For this purpose a coil of the nickel wire was wound non-inductively on a short brass bobbin provided with wide flanges. The plane of the coil was placed at right angles to the lines of force, so that the whole of the wire was magnetised transversely. Very strong fields were obtainable on account of the small thickness of the coil, 3 mm. only. It is impossible to measure the magnetic field which exists in the metal in such a case, but the field undisturbed by the nickel coil was determined for each measurement of the

resistance change. The nickel coil was subjected to the demagnetising process before each reading.

The variation of resistance is shown in fig. 7.

FIG. 7.



In weak fields, $\Delta\phi$ has a small positive value, but a reversal takes place in a field of 1500 c.g.s. For higher fields $\Delta\phi$ is negative and increases continuously; even in the highest field, H = 28,000, there is no indication of $\Delta\phi$ having a definite limit. The general form of the curve is similar to the curves obtained by Beattie for certain nickel films.

From these results it was found that the decrease observed in Experiment II might be accounted for by assuming that about one-fifth of the wire was transversely magnetised. The slight decrease observed in Experiment I is also to be explained in the same way.

Experiment IV.—Transverse Effect in Nickel.

A flat spiral of the nickel wire was inclosed between two plates of mica. Using the fine pole pieces at a distance apart of only 1 mm., the following result was obtained:—

$$\Delta\phi = -0{\cdot}0199, \qquad H = 34{,}000 \text{ c.g.s.}$$

The field strength was determined before placing the spiral in the field. This value of the resistance change is the greatest effect observed in these experiments.

Experiment V.—Transverse Effect in Iron.

An attempt was also made to measure the transverse effect in iron, using the same method as in Experiment III. The effect observed

Table I.—Dimensions of Wires used in the Experiments.

Experiment.	Diameter of wire in millimetres.	Resistance. Ohms per centimetre. 15° C.	Actual resistance of coil in ohms. 15° C.	Total length of wire in centimetres.
Ordinary fields—				
Iron	0·330	0·01450	1·83	127
Steel	0·765	0·00382	1·01	263
Nickel	0·765	0·00217	0·573	264
High fields—				
Nickel. Exp. I ..	0·200	0·02900	6·90	—
,, ,, II ..	,,	,,	1·31	—
,, ,, III ..	,,	,,	4·65	—
,, ,, IV ..	,,	,,	0·57	—
Iron ,, V ..	—	—	1·40	—

Table II.—The Longitudinal Effect for Nickel, Iron, and Steel.
(Ascending reversals for mean temperature 10° C.)

H.	Nickel.		Soft iron.		Steel.	
	I.	$\Delta\phi \times 10^{-6}$.	I.	$\Delta\phi \times 10^{-6}$.	I.	$\Delta\phi \times 10^{-6}$.
10	10	—	680	50	70	—
20	60	100	1040	210	800	—
30	178	550	1180	340	1150	300
40	225	1250	1230	440	1230	470
50	256	1900	1270	520	1260	545
60	277	2450	1300	590	1280	600
80	305	3500	1340	700	1320	710
100	327	4400	1370	800	1360	823
150	366	6300	1430	1040	1420	1055
200	393	8050	1470	1240	1470	1270
250	411	9400	1500	1400	1500	1460
300	426	10500	1530	1550	1530	1640
350	436	11400	1560	1670	1560	1780
400	446	12300	1580	1760	1580	1920
450	456	12900	1590	1850	1600	2030
790*	474	} 17000				
11000*	—					
Resid. (max.)	250	900	840	70	1120	180

Note (*) refers to Experiment I in strong fields.

was very small, but, as in the case of nickel, exhibited a change of sign, the reversal taking place in a field of 9500 c.g.s. (See fig. 7.)

The highest field obtained was only 11,000 c.g.s., and as the self-demagnetising force must be very great in this case, it is possible that with much stronger fields the effect may increase rapidly. The same specimen of iron was used as in the experiments with ordinary fields.

All the experiments described above were carried out in the Physical Laboratory of the University College of North Wales; and, in conclusion, I desire to acknowledge my great obligation to Professor E. Taylor Jones for the interest he has taken in the work, and also for much valuable help and advice.

"Influence of Temperature on the Conductivity of Electrolytic Solutions." By W. R. BOUSFIELD, M.A., K.C., M.P., and T. MARTIN LOWRY, D.Sc. Communicated by Professor H. E. ARMSTRONG, F.R.S. Received and read June 19, 1902.

The phenomenon of electrolysis is characteristic mainly of the liquid state, a liquid electrolyte usually ceasing to conduct when it passes into the gaseous or into the crystalline state. The influence of temperature on the conductivity of a liquid such as an aqueous solution of hydrogen chloride is, however, of such a character as to indicate that an upper and a lower limit of conductivity may exist apart altogether from the boiling point and freezing point of the solution. The present communication contains a summary of the evidence for the existence of these limits of conductivity, a brief discussion of their probable position on the scale of temperature in the case of some aqueous and other electrolytes, and a review of the influence of temperature on conductivity over the whole range of temperature within which electrolysis can take place.

Whatever view be taken of the nature of the process by which a conducting solution is formed on dissolving a salt, acid, or base, in an "ionising solvent," there is every reason to believe that the process is only complete in presence of a very large excess of solvent, and that usually only a part of the solute is concerned in carrying the current. The proportion of the solute that is thus rendered active in electrolysis is represented by a "coefficient of ionisation," and two general methods are in use for determining its magnitude. In the first method, the "equivalent conductivity," λ, of the solute is determined for a series of dilutions, and the ratio $\lambda v/\lambda_\infty$ of this constant at a dilution of v litres per equivalent to that at infinite dilution is taken to represent the coefficient of ionisation at volume v. This method is based on

the assumption that the "mobility" of the ions which carry the current is independent of the dilution, and is probably subject to serious errors when applied to solutions containing more than one equivalent of solute in 100 litres. The second method is based on the determination (usually from the boiling point or freezing point) of the osmotic pressure of the solution. The "active" part of the solute produces an osmotic pressure n times as great as that produced by an equimolecular quantity of an "inactive" solute, n being the number of ions into which the molecules of the solute would be decomposed on electrolysis. The coefficient of ionisation is deduced from the equation $\alpha = \frac{i-1}{n-1}$, in which the factor i represents the ratio of the observed osmotic pressure to that calculated for an inactive solute at equal dilution. This method of measuring α is also valid only in dilute solutions, is probably subject to error when applied to solutions containing more than one equivalent of solute in 10 litres.

In the majority of cases the magnitude of the coefficient of ionisation is found to decrease as the temperature rises, and the primary effect of an increase of temperature is therefore to reduce the amount of active material in the solution—an effect which may be attributed to the gradual disappearance of the "ionising power" of the solvent. But whilst the coefficient of ionisation usually decreases as the temperature rises, the equivalent conductivity at infinite dilution invariably increases. This is due to an increase in the "mobilities," u and v, of the kathion and anion, which together make up λ_∞, and is intimately related to the decreasing viscosity of the solution, which allows the migration of the ions to proceed more rapidly as the temperature rises. The effect of temperature on the equivalent conductivity of an electrolytic solution is therefore determined by two opposing influences, and the temperature coefficient $\frac{1}{\lambda}\frac{d\lambda^*}{d\tau}$ will be $-$ or $+$ according as one or other of these influences predominates.

In the case of aqueous solutions, the temperature coefficient at 18° C. is always positive, and usually amounts to about 2 per cent. of the conductivity at 18° per degree Centigrade. The conductivity temperature curves are very flat (compare fig. 1), and have frequently been represented by linear formulæ such as $\lambda_t = \lambda_0 (1 + \alpha t)$, but may often be more accurately represented by a parabolic formula such as $\lambda_t = \lambda_0 (1 + \alpha t + \beta t^2)$. If the linear or parabolic curves be produced in the direction of decreasing temperature, they would cut the axis

* The temperature-coefficient $\frac{1}{\kappa}\frac{d\kappa}{d\tau}$ of specific conductivity is not identical with $\frac{1}{\lambda}\frac{d\lambda}{d\tau}$; $\frac{1}{\lambda}\frac{d\lambda}{d\tau} = \delta + \frac{1}{\kappa}\frac{d\kappa}{d\tau}$ approximately, where δ is the coefficient of expansion of the solution.

of temperature at points not more than 50° below the freezing point of water. The point of intersection is an important constant

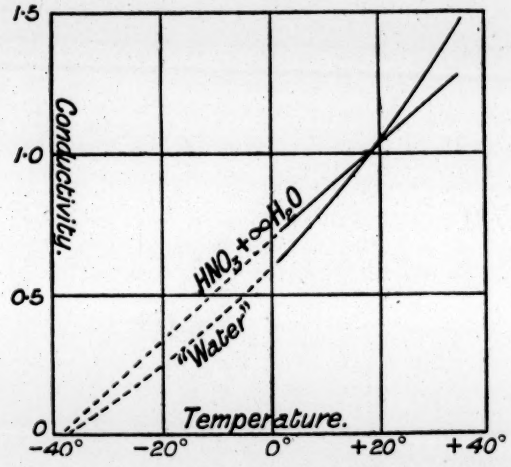


FIG. 1.—Influence of temperature on the conductivity of very dilute solutions.

for any given solution, and we have been accustomed to refer to it as the *conductivity zero* of the solution. At this temperature, if the observed relationship between conductivity and temperature should continue to hold good, the conductivity of the overcooled solution would become zero, and the conductivity zero may therefore be satisfactorily compared with the absolute zero of a gas thermometer at −273°.

The influence of temperature on the conductivity of aqueous solutions has been investigated by Grotrian, Kohlrausch, Arrhenius, Déguisne,* and others. In an important paper which has recently appeared,† Kohlrausch has deduced from Déguisne's measurements the values of the temperature coefficient in aqueous solutions of infinitely great dilution, and has arrived at the important conclusion that the conductivity temperature curves for all such solutions would, if produced, cut the axis of temperature at points lying within a degree or two of −38·5° C., the differing slope of the lines being compensated by their differing curvature (fig. 1). This temperature, which is almost independent of the nature of the solute, is evidently a fundamental constant of the solvent, and may be referred to as the "*conductivity zero of the solvent.*"

The physical meaning of the conductivity zero is a matter of some importance. Kohlrausch states that the viscosity of water may be represented by the formula $\eta = 2{\cdot}989\ (t + 38{\cdot}5)^{-1{\cdot}40}$, which leads to an infinitely great viscosity at −38°·5, and suggests that at this

* 'Diss., Strassburg,' 1895.

† 'Sitz. Preuss. Akad. Wiss.,' vol. 42, p. 1026, 1901.

"critical temperature" the water would, "independently of crystallisation reach a lower limit of the liquid state," the mobility of the ions being simultaneously reduced to zero. The actual existence of such a sharply defined critical temperature appears to us to be somewhat improbable. We consider it more likely that in an overcooled solution the relationship between conductivity and temperature, determined from observations above the freezing-point, would cease to hold good in the neighbourhood of the conductivity zero, and that even at considerably lower temperatures the electrolyte might still retain some appreciable conductivity.

We have obtained experimental evidence in support of this view from a study of the conductivity of glass, which may be looked upon as a typical example of an overcooled electrolyte. The accompanying curve and table (fig. 2) represents a series of observations which we

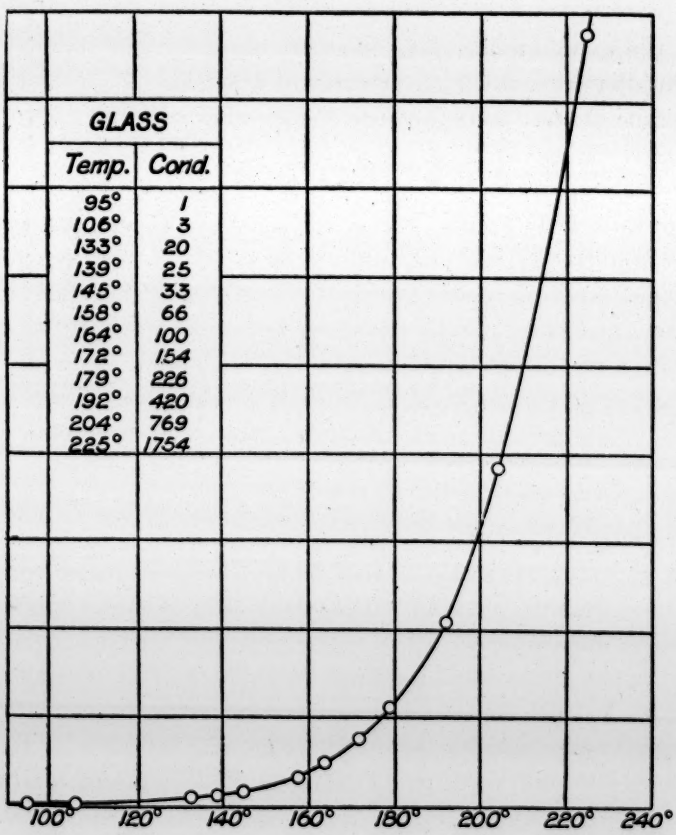


FIG. 2.—Influence of temperature on the conductivity of glass.

have made on the change with temperature of the conductivity through the walls of a glass test-tube. The test-tube was half-filled with mercury, and was placed inside a larger tube, also containing mercury, and heated in a sand-bath; the inner tube contained a thermometer,

and the conductivity between the two layers of mercury was determined by a potentiometer method; the conductivities are expressed in arbitrary units. Above 200° the curvature is very slight and the curve is similar to that of an aqueous electrolyte above 0°. Extrapolation from this part of the curve would indicate the existence of a "conductivity zero" at about 185° C., but as this temperature is approached the decrease of conductivity becomes much less rapid, the "critical temperature" is passed without any abrupt change in the curve, and even below 100° the conductivity is still measurable.

Further evidence of a similar character is supplied by the temperature-viscosity curves. The formula given by Kohlrausch leads to a limit of fluidity at $-38°{\cdot}5$, but no statement is made as to the experimental data on which it is based. Thorpe and Rodger* give two formulæ for the viscosity of water. The first,

$$\eta = 5{\cdot}9849(43{\cdot}252 + t)^{-1{\cdot}5423},$$

expresses the experimental data from 0° to 100°, and would lead to a limit of fluidity at $-43°{\cdot}2$, a temperature within 5° of Kohlrausch's critical temperature. But the second formula,

$$\eta = 58{\cdot}7375(58{\cdot}112 + t)^{-1{\cdot}9044},$$

which expresses with accuracy the viscosity of water between 0° and 8°, would lead to a limit of fluidity at $-58°{\cdot}1$, that is, 20° below Kohlrausch's "critical temperature."† In this case, therefore, a change in the law governing the relationship between viscosity and temperature is already noticeable above the freezing point of water, and is of such a character as to lead to a "fluidity zero" considerably below the "conductivity zero" deduced from measurements between 2° and 34° C.

We conclude therefore that, whilst the conductivity zero at $-39°$ C. is an important physical constant of water, it is a measure of the properties of water between 2° and 34° only (the temperature limits of Déguisne's conductivity measurements), and not an actual critical temperature comparable with the freezing point. In accordance with this view we have, in summarising the influence of temperature on conductivity (fig. 3), represented the conductivity as persisting at a temperature T_1 considerably below the conductivity zero at T_2.

To express the relationship between conductivity and temperature, Déguisne and Kohlrausch employ the formula

* 'Phil. Trans.,' A, 1894, vol. 185, pp. 397—710.

† A still more striking illustration of the influence of the experimental temperatures on the fluidity-limit is afforded by the case of active amyl alcohol (Thorpe and Rodger, *loc. cit.*, p. 542). The viscosity of this is represented by three different formulæ, which hold good from 0° to 35°, 35°—73°, and 73°—124°, and would lead to zero values for the fluidity at $-101°$, $-65°$, and $-8°$ respectively.

$$\kappa_t = \kappa_{18}\{1 + \alpha(t-18) + \beta(t-18)^2\}.$$

In the case of solutions of very great dilution, the coefficient β, which determines the curvature of the lines (fig. 1) is related to the coefficient α, which determines the slope of the tangent at 18° C., in accordance with the equation

$$\beta = 0{\cdot}0163(\alpha - 0{\cdot}0174),$$

and the whole influence of temperature on conductivity is thus determined by a single arbitrary constant. A similar equation is also used by Kohlrausch to express the influence of temperature on the separate ionic mobilities. Negative values of β occur only in the case of the hydrogen ion,

$$\alpha = +0{\cdot}0154, \qquad \beta = -0{\cdot}000033,$$

and the acids which contain that ion; positive values of β range from that of the hydroxyl ion,

$$\alpha = +0{\cdot}0179, \qquad \beta = +0{\cdot}000008,$$

which obeys a law which is almost linear to that of the lithium ion,

$$\alpha = +0{\cdot}0261, \qquad \beta = +0{\cdot}000155,$$

where the curvature of the lines in the conductivity temperature diagram is very pronounced. In order to make a direct comparison between conductivity and viscosity—a comparison which is hardly possible when two entirely different types of formula are used for the two phenomena—we have sought to express the viscosity of water by means of the formula

$$\eta_{18} = \eta_t\{1 + \alpha(t-18) + \beta(t-18)^2\}.$$

In order to make the comparison as exact as possible, we have deduced the constants α and β from the values given by Thorpe and Rodger for the viscosity of water at 5°·45, 13°·52, and 30°·72, the temperature limits being thus almost identical with those of Déguisne's experiments, and have thus obtained the values

$$\alpha = +0{\cdot}0251, \qquad \beta = +0{\cdot}000115,$$

for the two constants. We find that between 5° and 55° this expression represents the experimental data with even greater closeness than the formula used by Thorpe and Rodger, but that above 55° it gives values smaller than those determined experimentally. The closeness of the agreement is shown by the following table:—

t.	η (obs.).	η (calc.) (Thorpe and Rodger.)	Diff.	η (calc.) (Bousfield and Lowry.)	Diff.
5·45	0·01494	0·01494	0	0·01494	0
13·52	0·01181	0·01179	−2	0·01181	0
22·02	0·00955	0·00951	−4	0·00954	−1
30·72	0·00786	0·00784	−2	0·00786	0
39·32	0·00662	0·00661	0	0·00662	0
47·03	0·00576	0·00577	+1	0·00576	0
55·53	0·00501	0·00502	+1	0·00500	−1

The values of α and β in this formula for the viscosity of water are considerably larger than those which express the influence of temperature on the conductivity of the majority of aqueous solutions, but *agree remarkably closely with the values given by Kohlrausch for the conductivity of purified water*; this he found to have an abnormally large temperature coefficient, the constants being

$$\alpha = + 0{\cdot}0254, \qquad \beta = + 0{\cdot}000130,$$

as compared with the values of the constants in the viscosity formula

$$\alpha = + 0{\cdot}0251. \qquad \beta = + 0{\cdot}000115.$$

The striking agreement between the constants in the two equations shows that not only do the conductivity and viscosity of water tend towards the same limiting temperature, but also that their variation with temperature can be expressed by one formula and represented by one curve; between 2° and 22° the maximum divergence between the values calculated from the two formulæ is only 0·2 per cent.

The conductivity zero for water at − 39° is not an isolated phenomenon, and although the data available are in no case so numerous or so accurate as in the case of water, it is possible to make at least an approximate determination of the position of the conductivity zero for a number of other solvents. For this purpose three different methods are available.

(1.) The influence of temperature on viscosity has been determined with great care by Thorpe and Rodger for a large number of liquids, several of which are capable of acting as ionising solvents. In the case of these solvents the conductivity zero should lie at temperatures not far removed from the limits of fluidity deduced from the viscosity formulæ. In this way an approximate determination can be made of the conductivity zero of the following solvents:—

Acetone	− 210° C.
Ethyl ether	− 136
Methyl alcohol	− 164
Ethyl alcohol.....	− 210

In no case is the temperature coefficient of viscosity so great as in the case of water, and the conductivity zero for each of these liquids is evidently very far below the conductivity zero for water.

(2.) At great dilutions the influence of temperature on ionisation is usually much reduced, and aqueous salt solutions of N/1000 strength have temperature coefficients of conductivity which do not differ appreciably from those calculated for solutions of indefinitely great dilution. These coefficients can be used in determining the conductivity zero of the solvent, and a rough estimation is possible from measurements made in much less dilute solutions. The conductivity of a number of salts dissolved in liquid ammonia has been determined by Legrande* at temperatures ranging from $-32°$ to $-70°$ C., the dilution in each case being greater than 100 litres per equivalent. The mean temperature coefficient at $-60°$ C. for the five salts was $+0{\cdot}012$, corresponding with a conductivity zero at $-140°$. A similar examination of Walden and Centnerszwer's measurements of solutions in liquid sulphur dioxide between 0° and $-70°$ C.† serves to place the conductivity zero for this solvent at about $-160°$ C. The measurements of Dutoit and Friedrich‡ of the conductivity between 0° and 25° of solutions of AmCNS and NaI in acetonitrile lead to a conductivity zero for the nitrile at $-90°$ C.

(3.) The temperature coefficient of conductivity of purified water leads to a satisfactory value for the conductivity zero. By applying a similar method to Fresnel's measurements of the conductivity between $-40°$ and $-80°$ of purified ammonia§ we have obtained for the conductivity zero the value $-130°$, a value which is substantially in agreement with that deduced from the behaviour of Legrande's salt solutions.

Whilst at low temperatures the effect of temperature on conductivity is determined mainly by the changing mobility of the ions, at higher temperatures the changing ionisation becomes the dominant factor. And just as the increasing viscosity of the solution leads at low temperatures towards a *lower* conductivity zero at which the viscosity of the liquid would altogether prevent electrolysis, so at high temperatures an *upper* limit may exist at which the conductivity would again become zero owing to the complete disappearance of ionisation.

In the case of aqueous solutions the indications of an *upper conductivity zero* are only slight. At 18° the temperature coefficients of conductivity are all positive, and only in very exceptional cases have negative coefficients been observed, even at higher temperatures.

* Thesis, Paris, 1900.

† 'Zeit. Phys. Chem.,' 1902, vol. 39, p. 542.

‡ 'Bul. Soc. Chim.,' 1898, vol. 19, pp. 321—337.

§ 'Zeit. Electrochem.,' 1900, vol. 6, p. 485.

Nevertheless, when the influence of changing ionic-mobility is eliminated, and the coefficient of ionisation is dealt with separately, the decay of ionisation as temperature rises is in many cases very clearly marked, even at 0° C., and in presence of a large excess of the solvent. Thus Whetham* has shown that at a dilution of 1000 litres per molecule of solute the coefficient of ionisation of potassium chloride falls from 0·992 at 0° to 0·982 at 18°, and that of barium chloride from 0·969 at 0° to 0·954 at 18°; in the case of copper sulphate no decrease occurs at dilutions greater than 1000 litres, but at a dilution of 100 litres the coefficient falls from 0·638 at 0° to 0·623 at 18°. These and other measurements that have been made between 0° and 100° serve, however, mainly to show that within these limits the coefficient of ionisation is influenced only to a relatively small extent by temperature, and that the upper conductivity zero must lie very much further from the boiling point than the lower zero is removed from the freezing point.

The different behaviour of acids and salts, which is unimportant in the case of the lower conductivity zero, becomes a vital factor in discussing the possible existence of an upper conductivity zero. Armstrong† has already called attention to the importance of the fact that whilst the majority of salts are conductors *per se*, the acids are, in the pure state, dielectrics, and only become electrolytes by interaction with an "ionising solvent." In virtue of their inherent power of "*self-ionisation*" the salts may therefore exhibit some conducting power independently of that due to the ionising properties of the solvent, whilst it is to be anticipated that the acids would cease to be electrolytes if the solvent, by reason of increasing temperature, should lose its power of ionisation. For this reason evidence of the existence of an upper conductivity zero is to be looked for especially in solutions of substances which like the acids are incapable of self-ionisation. The acids are also especially suited for this purpose owing to the fact that the mobility of the hydrogen ion increases relatively slowly with rising temperature. Even at atmospheric temperatures the temperature coefficients for the acids, though positive, are relatively small, and the acids would therefore be the first to exhibit a maximum of conductivity and a reversal in the sign of the temperature coefficient. This effect of decreasing ionisation was actually observed by Arrhenius in the case of phosphoric and hypophosphorous acids, which exhibit maxima of conductivity at 54° and 75° respectively, and maxima of conductivity have also been observed by Sach‡ in the case of aqueous solutions of copper sulphate. In the majority of aqueous solutions the maximum of conductivity lies above the

* 'Phil. Trans.,' 1900, vol. 194, pp. 321—360.

† 'Roy. Soc. Proc.,' 1886, vol. 40, p. 268.

‡ 'Wied. Ann.,' 1891, vol. 43, p. 212.

boiling point and experimental work is rendered very difficult, owing to the ready solubility of glass in water at high temperatures; this not only contaminates the solutions but greatly weakens the sealed tubes in which they are contained. Hagenbach* has, however, made a single observation which is of importance as showing that even the salts of the alkali-metals may exhibit negative temperature coefficients at high temperatures; in the case of an aqueous solution of KCl, containing dissolved glass, he actually observed on one occasion a maximum of conductivity before the explosion of the tube took place at 310°.

Much more evidence is available for the existence of an upper conductivity zero in the case of non-aqueous solutions. Franklin and Kraus† have found that at high temperatures the conductivity of solutions in liquid ammonia decreases as the temperature rises—an effect directly opposite to that observed by Legrande at low temperatures; and Maltby‡ has shown that even at atmospheric temperatures the conductivity of an ethereal solution of hydrogen chloride decreases as the temperature rises, and in the neighbourhood of the critical temperature is only $\frac{1}{38}$ of the conductivity at 18°. Negative temperature coefficients have also been observed by Cattaneo§ in solutions in ether, alcohol and glycerol.

The most valuable experimental data, however, are those derived from the study of solutions in liquid sulphur dioxide. Hagenbach,‖ in order to ascertain whether the limit of conductivity was reached at the critical temperature, measured the conductivity of solutions in sulphur dioxide at temperatures ranging from 20° to 160°, and found that the temperature-coefficients between 100° and 140° of KCl, KBr, KI, and NaI were all negative, and amounted to about 2 per cent. of the conductivity at 100° for each degree Centigrade. The upper conductivity zero of these solutions, determined by extrapolation from the measurements between 130° and 150°, would lie in each case some 5° or 10° above the critical temperature of the solution, but immediately below the critical temperature the conductivity falls rapidly over a narrow range of temperature until it reaches the small but measurable conductivity of the gas. It is to be noticed that the substances examined by Hagenbach are all self-ionising—this would retard the decay of ionisation, and would have the effect of raising the upper temperature-limit of conductivity. High temperature measurements have also been made by Walden and Centnerszwer¶ in the case

* 'Ann. d. Physik,' 1901 [5], vol. 2, p. 306.
† 'Amer. Chem. Jour.,' 1900, vol. 24, p. 83.
‡ 'Zeit. Phys. Chem.,' 1895, vol. 18, p. 133.
§ 'Rend. Lincei' [5], vol. 2, I, p. 295, and II, p. 112, 1893.
‖ 'Ann. d. Physik,' 1901 [5], vol. 2, pp. 276—312.
¶ 'Zeit. Phys. Chem.,' 1902, vol. 39, p. 549.

of solutions in sulphur dioxide of hydrogen chloride, quinoline, and a number of organic iodides. The first of these solutions is of special interest, since hydrogen chloride is known to be of itself a non-electrolyte, and to possess no power of self-ionisation; the conductivity of its solution in sulphur dioxide, unlike that of the salt-solutions examined by Hagenbach, appears to decrease regularly to a zero value at the critical temperature, which is therefore identical with the upper conductivity zero of the solution.

We are now in a position to review the influence of temperature on the conductivity of a "composite electrolyte" over the whole range of temperature within which it remains a conductor. Its general character may be represented by means of a curve (fig. 3), in which tempera-

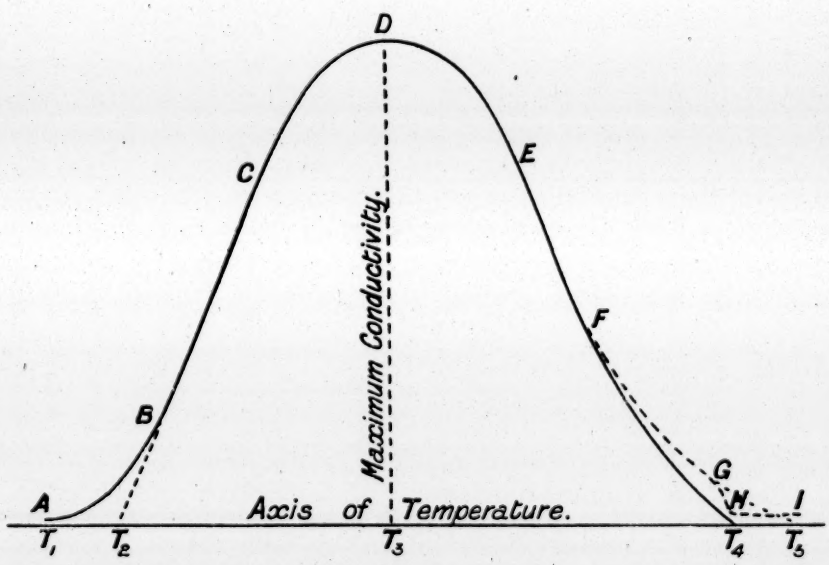


FIG. 3.—General scheme, showing the influence of temperature on the conductivity of a composite electrolyte.

tures are represented as abscissæ on a horizontal axis and conductivities as ordinates. At some intermediate temperature T_3, depending on the nature of the solvent and solute as well as on the concentration of the solution, the conductivity reaches a maximum and the temperature coefficient is momentarily zero. As the temperature falls the conductivity decreases, the increasing viscosity more than counterbalancing the effects of increasing ionisation. Over a considerable range, BC, the curve follows an approximately linear law, the line becoming concave to the axis of temperature near C, and convex near B. On this part of the curve the conductivity of the majority of aqueous solutions must be represented, the acids giving values on the concave and the salts on the convex part of the curve. This portion of the curve would, if produced, cut the horizontal axis at T_2, the

lower conductivity zero of the solution; but as this temperature is approached the curve probably turns aside and becomes asymptotic to the axis of temperature.

Above the temperature of maximum conductivity the conductivity also decreases, the decreasing viscosity being now more than counterbalanced by the decreasing ionisation of the solution. The decay of ionisation becomes more rapid as the temperature rises, and if the solute is not an electrolyte *per se*, the curve EF runs steadily down and cuts the axis at T_4, the critical temperature of the solution, which is thus the upper conductivity-zero of the solution. In the case of very dilute solutions this would, of course, be identical with the critical temperature of the solvent. If, however, the solute is capable of self-ionisation, the curve EFG tends towards an upper conductivity zero at T_5 a few degrees above the critical temperature, but as the critical temperature is approached the conductivity falls abruptly along GH to a value comparable with that which persists in the gaseous state HI.

[*Note added August* 2, 1902.—The general scheme of fig. 3 serves to bring into prominence at least one important point that has been very generally overlooked, namely, that *the normal form of the conductivity-temperature curve for a composite electrolyte is one which contains a point of inflexion*. Two such points are shown in fig. 3, between B and C and between E and F. Of these, the former should be frequently observed in aqueous solutions, seeing that these give values lying on parts of the curve both above and below the inflexion. A widespread impression exists, however, that an inflexion in the conductivity-temperature curve indicates some abnormal change in the character of the solution. This impression has been strengthened, if indeed it has not been created, by the general adoption of a linear or parabolic formula to express the influence of temperature on conductivity. Kohlrausch, in his detailed review of the literature of the subject,* makes no reference to the existence of inflected curves, and does not even hint at the possibility of curves of this type. Trötsch, who observed inflexions in the case of a number of sulphates and the chlorides of copper and cobalt,† regarded them as due to the decomposition of hydrates existing in the solution, whilst Donnan and Bassett, in a paper which has only just appeared,‡ quote the inflexion observed by Trötsch as evidence for the existence of a complex ion in solutions of cobalt chloride.

According to the views here put forward, the conductivity-temperature curves are all inflected. In the case of the acids, which owe their conducting power entirely to the action of the solvent, the inflexion

* 'Leitvermögen der Elektrolyte,' pp. 116–123 and 195–199.

† 'Ann. Phys. Chem.,' 1890 [iii], vol. 41, pp. 259–287.

‡ 'Jour. Chem. Soc.,' vol. 81, p. 953, August, 1902.

lies below 0°; in the case of the alkalies our own experiments have shown that the curves, which have usually been represented as straight lines or flat parabolas, all exhibit inflexions, in the case of the alkalies, LiOH, NaOH and KOH at about 40°, and in the case of the alkaline earths, $Ca(OH)_2$, $Sr(OH)_2$ and $Ba(OH)_2$, at about 25°; in the case of the less highly ionised salts, such as magnesium sulphate, the inflexion also lies below 100°, but in the case of salts such as potassium chloride, which have a high coefficient of ionisation in solution and are also electrolytes *per se* in the fused state, the inflexion lies above the boiling-point of the solution.]

"On the Measurement of the Bactericidal Power of Small Samples of Blood under Aerobic and Anaerobic Conditions, and on the Comparative Bactericidal Effect of Human Blood drawn off and tested under these Contrasted Conditions." By A. E. WRIGHT, M.D., Professor of Pathology, Army Medical School, Netley. Communicated by Professor J. R. BRADFORD, F.R.S. Received April 7, 1902. Received in revised form August 5, 1902.

SECTION I.—*Method of Measuring the Bactericidal Power of the Blood under Ordinary (Aerobic) Conditions.*

As a preliminary to placing on record certain observations made in connection with the bactericidal power of human blood, I propose to describe in detail the technique which has been elaborated by me with a view to carrying out these and similar investigations.

A measurement of the bactericidal power of the blood involves in the first place a standardisation of bacterial culture employed.

I. *Standardisation of the Bacterial Culture employed.*

A standardisation which would seem to satisfy all practical requirements can be achieved (*a*) by employing in the course of a series of experiments one and the same stock of bacteria; (*b*) by employing in each experiment a young (*e.g.*, a 24 hour old) culture; (*c*) by determining in each case the number of living bacteria contained in a measured volume of that culture.

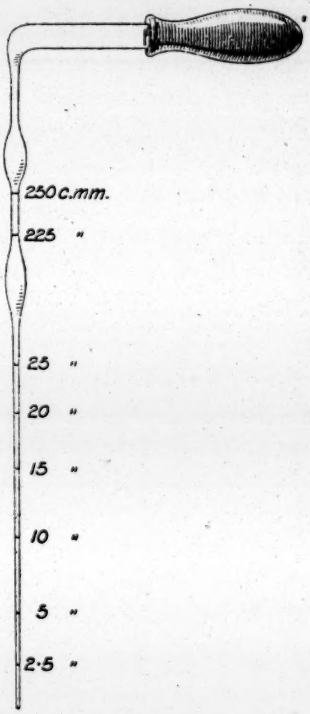
The determination last mentioned* involves—*first*, the making of a

* (Added 3.8.02.) Where, in lieu of the number of living bacteria, the total number of bacteria in a culture is to be determined, this can be directly determined under the microscope by the method I have described in the 'Lancet' of July 5, 1902.

measured dilution of the culture (in the case of an ordinary bacterial culture, dilutions of 1,000,000 fold to 10,000,000 fold are appropriate), and, *secondly*, the transference—with a view to the subsequent enumeration of the colonies which develop—of a series of measured volumes of the diluted culture to the surface of a solid nutrient medium.

The processes of diluting and measuring off the desired volumes of diluted culture can be conveniently and unlaboriously carried out by means of the diluting pipette figured below. (Fig. 1).

FIG. 1.



(1.) *Method of making and calibrating the "Diluting Pipette."*

(*a.*) A piece of glass tubing about 15 cm. long is drawn out at one end into a capillary stem.

(*b.*) A standard 5-cm. pipette is fitted with a rubber teat,* and is then filled in up to the calibration mark with mercury. The 5 cm. of mercury is now transferred from the capillary pipette to the wide end of the glass tube, and is made to enter the upper portion of the capillary stem.

(*c.*) When this has been effected, the points corresponding to the upper and lower ends of the mercury column are marked off on the outside of the capillary stem with a coloured pencil, preferably one of the oil pencils sold for writing on glass.

(*d.*) The mercury column is now displaced downwards until the upper end of the column stands opposite the lower of the calibration marks. This point is again marked off with the coloured pencil.

(*e.*) By a similar procedure, three more marks indicating divisions of similar value are placed on the outside of the capillary stem.

(*f.*) This done, the tube is filed and broken off at the lowest mark.

(*g.*) It will now be convenient to divide the lowest 5-cm. division into two divisions of 2·5 cm. This can be conveniently effected by the following method of trial and error :—Mercury is drawn up into the tube until the upper limit of the column of metal stands as nearly as possible midway between the orifice and the first 5-cm. division mark. The point corresponding to the proximal end of the mercury column

* A mechanically controlled teat, such as that made by Mr. A. E. Dean, jun., 73, Hatton Garden, E.C., is a convenient form to employ for this purpose.

may now be tentatively indicated on the outside of the capillary stem by a light pencil mark. This done, the column of mercury is displaced until its proximal end stands level with the 5-cm. division mark. If the distal end of the mercury column now coincides with the tentative subdivision mark, this last gives the desired 2·5-cm. division. If it does not coincide, the desired point will be situated half-way between the point now indicated by the proximal extremity of the mercury column and the point indicated by the original trial subdivision mark.

(*h.*) What has been achieved up to this point is a graduation of the capillary stem into five divisions of 5 cm., and a subdivision of the first of these into two 2·5-cm. divisions. A further process of graduation in terms of 25 cm. is now taken in hand, with a view to finding the points corresponding respectively to 225 and 250 cm.

(*i.*) For this purpose a rubber teat is placed upon the upper end of the tube, and a negative pressure having been established, the capillary stem is filled up to the 25-cm. mark with mercury, water, or a coloured fluid. It is then filled in succession with eight further 25-cm. volumes, the 25-cm. volumes being in each case spaced off from each other by a bubble of air. After these air bubbles have risen to the surface in the wide upper portion of the tube, and the separate volumes have here united to form a single body of fluid, a mark is placed on the outside of the tube to indicate the 225-cm. point. An additional 25-cm. volume of fluid is now introduced, and the point corresponding to 250 cm. is similarly registered.

These last marks, be it noted, serve only for the provisional graduation of the tube.

(*j.*) With a view to achieving a more accurate graduation, the portion of the glass tube between the 225 and 250 cm. graduation marks is fused in the blow-pipe flame, and is drawn out into a short thick capillary tube such as will admit of a more accurate calibration.

(*k.*) The calibration in terms of 25 cm. is now repeated, and the points corresponding to 9 and 10 multiples of 25 cm. are now finally marked off on the narrow portion of the tube.

(*l.*) A safety chamber is formed on the upper part of the tube, the wide end of this last being carried round at right angles to the stem to allow of more convenient manipulation.

(*m.*) Lastly, the pencil marks are carried round the whole circumference of the capillary tube, and they are fixed upon the glass by passing them through the flame.

(2.) *Method of employing the Capillary-diluting Pipette.*

By means of a diluting pipette fitted with a rubber teat any desired dilution of the culture can be obtained very unlaboriously.

A *ten-fold dilution* of the culture—the dilution which is perhaps most often required—is made by taking first 25 cm. of the culture, and then, after the interposition of an air bubble, filling up to the 250-cm. mark with sterile broth. It can also, and this avoids any contamination of the sterile diluting fluid, be made by filling up first with 225 cm. of the broth, and completing up to the 250-cm. mark with the culture.

A *six-fold dilution*, should such be required, would be obtained by filling in to the 250-cm. mark with sterile broth, and then completing with two volumes of culture, these last being isolated as before by intervening air bubbles.

A *five-fold dilution* is obtained by filling in with two separate 25-cm. volumes of the culture, and completing up to the 250-cm. mark with sterile broth.

A *two and a half-fold dilution* would be obtained by filling in with four separate volumes of 25 cm., and completing up to the 250-cm. mark with sterile broth.

Dilutions of a different order can be obtained by filling in the pipette as occasions may require with 2·5 or 5 cm. of culture, and then completing to 250 cm. with sterile broth. By this means dilutions of 1 in 100 and 1 in 50 respectively can be obtained *uno saltu*.

By a series of successive dilutions, made in each case after washing out the pipette with boiling sterile water, any desired attenuation of the culture can be quickly arrived at. The dilution of 1 in 1,000,000 ordinarily required for the purposes of enumeration will be obtained by three successive dilutions of 1 in 100.

(3.) *Method of Eliciting the Number of Micro-organisms contained in the Diluted Culture.*

The required dilution of, let us say, 1 in 1,000,000 having been prepared, the pipette would, after sterilisation in boiling sterile water, be filled in with, say, three successive 10 cm. volumes of the diluted culture. A corresponding number of agar tubes having been taken in hand, the three 10 cm. volumes of diluted culture would be separately transferred to the surface of the nutrient medium, care being taken in each case to spread out the fluid over as large an area of surface as possible.

After incubation, the number of bacteria in each 10 cm. volumes of diluted culture would be deduced from the number of colonies which develop on the corresponding agar tube.

After averaging the number of bacteria contained in the three tubes, the number of living bacteria contained in 1 c.c. of the original culture would be found by a simple arithmetical process.

II. *Procedures in connection with the actual carrying out of the Bactericidal Estimation.*

In connection with the actual carrying out of the bactericidal estimation, we have to consider :—

(1.) The collection of the sample of blood for examination.

(2.) The preparation of a graduated series of dilutions of the bacterial culture.

(3.) The special form of capillary-testing pipette required for the subsequent procedures.

(4.) The method of employing the testing pipette just mentioned, *i.e.*, the method of mixing a series of measured volumes of serum with in each case an equal volume of the successive bacterial dilutions, and the method of determining the sterility or otherwise of the mixtures after the serum has acted upon the bacteria for an appropriate period.

(1.) *Collection of the Sample of Blood.*

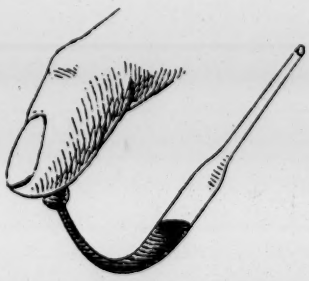
The quantity of blood required for an ordinary bactericidal estimation need never exceed 1 c.c.* Much more than the quantity required can, in the case of man, readily be obtained by driving the blood into the pulp of the finger by winding a handkerchief round the digit, making a prick with a needle or spicule of glass, and then making pressure on the pulp.

A convenient form of blood capsule is that figured below (fig. 2). The upper end of capsule, when drawn out in a peep flame or in the flame of a lucifer match, provides an aseptic pricker. When proceeding to collect the blood both this (A) and the end of the curved limb (B) are broken off. The blood then flows into the capsule, as shown in fig. 3, under the combined action of gravity and capillarity. When

FIG. 2.



FIG. 3.



* Where only minimal amounts of blood are available, the difficulty can be got over either by the employing very fine capillary tubes or by mixing progressive dilutions of the serum with one and the same dilution of the bacterial culture.

sufficient blood has been collected, the upper portion of the capsule is gently warmed and the upper orifice is then immediately sealed up. As the air, which has been rarefied by warming, contracts, the blood is drawn up into the body of the capsule, leaving the orifice at (B) free for resealing. After the capsule has cooled, it is suspended by means of its curved arm in a hand centrifuge, and the blood is driven down by a few turns of the handle into a previously upper half of the capsule. It is now left at rest for a few minutes. When the serum begins to exude, the centrifuge is again brought into action.

When the estimation is to be taken in hand, the neck of the curved bulb of the capsule is sterilised in the flame and is cut across with a stout pair of bone forceps, which has been sterilised in the same manner.

Two further points in connection with sample of blood may appropriately be considered. The first of these relates to the question of the necessity for aseptic precautions in drawing off the blood. The second to the question of the interval which may elapse between the drawing off of the blood and the bactericidal estimation.

The blood should be drawn off with aseptic precautions. For this purpose the surface of the finger may be readily and effectually sterilised by moistening it with alcohol, and burning this off.

Where results which are comparable among themselves are desired, the bactericidal estimations ought, in all cases, to be undertaken within a very few hours after the samples of blood have been withdrawn. Where samples of blood are tested immediately after withdrawal, and again after an interval of 24 hours, it is usual to find a notable diminution of bactericidal power in the second estimation.

(2.) *Preparation of a Series of Graduated Dilutions of the Bacterial Culture.*

It has already been indicated, but it will be well at this point clearly to bring out the fact, that in the method of bactericidal estimation here described, a series of measured volumes of undiluted serum are brought in contact with a series of graduated dilutions of the culture, the object being to determine what is the lowest dilution of the culture with which a complete bactericidal effect is exerted.

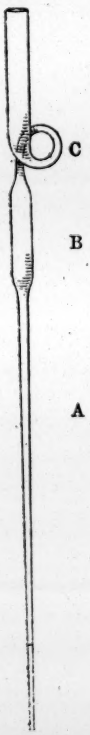
The graduated dilutions of the culture which are required for this purpose, so far as they have not already been provided by the procedure undertaken in connection with the enumeration of the bacterial culture, would, at this stage, be prepared by the aid of the diluting pipette. I have found it convenient in the case of the typhoid bacillus to employ, in addition to the undiluted culture, in each case a 2 fold, 5 fold, 10 fold, 25 fold, 50 fold, 100 fold, 1000 fold, 10,000 fold, and 100,000 fold dilution.

The dilutions are to be held ready for use in a series of covered sterile watch-glasses.

(3.) *Description of the Special Form of Capillary Testing Pipette Employed in the Connection with the Method of Bactericidal Estimation here described.*

Fig. 4 shows the particular form of capillary testing pipette which has been found most suitable. The stem A—it will be noted that it is provided with a pencil mark—serves on the one hand as a measuring pipette for measuring off equivalent volumes of serum and bacterial culture, and on the other hand as a receptacle for the combined volumes of fluid during the period allotted to the action of the serum upon the culture.

FIG. 4.



The bulb B functions, in the first stage of the procedure described below, as a receptacle for the sterile broth afterwards used for testing the continued vitality or otherwise of the bacteria which have been exposed to the influence of the serum. In the second part of the procedure, the bulb of the pipette comes into use as a cultivation chamber. The bulb may conveniently possess a capacity of about 1 c.c. The spiral C* serves to prevent any access of contaminating bacteria to the interior of the bulb.

(4.) *Method of Employing the Capillary Testing Pipettes.*

The method of employing the testing pipettes is as follows:—

A mark with the coloured oil pencil having been placed upon the capillary stem at any convenient point, say at a point 1—1·5 cm. from the lower end, a rubber teat is fitted over the upper end of the tube.

The point of the capillary stem is now broken off between finger and thumb, the lower portion is sterilised in the flame, and the air is expelled from the teat.

Sterile broth, which has been placed ready to hand in a covered sterile watch-glass, is then aspirated into the pipette until the bulb is about two-thirds full.

The extremity of the capillary stem is now withdrawn from the

* It will be found that the introduction of the spiral avoids the necessity for the troublesome plugging of the tube with cotton wool and the subsequent sterilisation process. The very simple trick of hand by which the spiral is made may be readily learned by imitating the motions associated with the making of a similar spiral upon a stiff cord or a very pliable wire.

broth, and the column of fluid which occupies it is allowed to run up very gently, so as to avoid any back lash, into the bulb. The inflow of air is arrested as soon as the capillary stem is empty of fluid.

(a.) *Method of Measuring off and Mixing together equal Volumes of Serum and Bacterial Cultures.*

The end of the capillary stem is now inserted into the narrow open end of the blood capsule, which has been placed ready to hand in a perforated rubber bung or other convenient receptacle. The serum is allowed to flow in until it reaches the pencil mark.

The orifice of the pipette is now raised above the surface of the serum, and a bubble of air is admitted into the tube to serve as an index for the next measurement.

This done, the end of the capillary stem is carried over into a watch-glass containing the particular dilution of the culture which is to be dealt with in this particular tube. The culture is allowed to flow in until the bubble of air has just been carried past the pencil mark.

The next procedure is to mix together the equal volumes of serum and culture which have been measured off. This is effected by blowing these two volumes out upon the surface of a sterile watch-glass—a pile of inverted sterile watch-glasses will for this purpose have been placed ready to hand—and drawing up and driving out the fluid several times in succession. After a little practice* this can be quite easily achieved without driving the sterile broth down from the bulb of the pipette into the lower part of the capillary stem and there contaminating it.

The column of mixed serum and culture is to be drawn up into the middle region of the capillary stem as a preliminary to sealing the lower end of the tube. It will be found that when the column is left in this position, the intervening column of air which occupies the upper portion of the capillary tube will effectually isolate the fluid in the bulb of the pipette for the mixture of serum and culture.

The teat is now removed, leaving the spiral to guard the contents of the tube against contamination, and the filling of the series of tubes with the remaining dilutions of the culture is proceeded with. When the whole series of tubes has been filled in, these are placed upright in a test tube labelled with the date and the source of the serum. The serum is then allowed to exert its influence on the bacteria with which it has been brought in contact for a fixed period at a fixed temperature.

* Until practice shall have conferred sufficient control over the teat, it will be advisable either to employ very fine capillary tubes or to provide a by-channel for the air by piercing the teat with a spicule of an extremely fine capillary tube.

I have found it convenient to allow the serum to remain in contact with the culture for a period of 18 to 24 hours at 37° C.

(b.) *Method of Testing the Continued Vitality or otherwise of the Bacteria which have been in Contact with the Serum.*

The sterile broth which has been filled into the capillary pipette furnishes, as we have seen, the means for determining whether the bacteria which have been brought in contact with the serum have or have not retained their vitality. If the serum has failed to kill the bacteria, this will be evidenced by the development of turbidity in the broth which will follow upon the aspiration of the column of fluid in the capillary stem into the bulb of the pipette. If, on the other hand, the serum has killed all the bacteria with which it has been mixed, the nutrient broth will, under the circumstances, remain clear.

The steps of the procedure are as follows:—

The tubes having been taken in hand singly, the lower portion of the capillary stem is in each case drawn out, and after heating in a peep-flame, into the finest possible filiform tube.

A condition of negative pressure is now established in the interior of the pipette by fitting over its upper end a collapsed rubber teat. While carefully regulating this negative pressure by keeping* the finger and thumb in position on the teat, the finely-drawn-out end of the capillary stem is gently snapped across. The column of fluid will then be very quietly carried up into the bulb of the pipette.

The determination of the continued sterility or otherwise of the broth may generally be made after incubation by mere naked eye inspection. Where a doubt arises either as to the existence of a growth, or as to the nature of the cultivation obtained, a drop of the culture may be microscopically examined or cultivated on nutrient agar.

III. *Method of Expressing the Results obtained by the Method of Bactericidal Estimation here in question.*

The question which is investigated by the method described above is, as has been seen, the question as to what is the lowest dilution of the particular enumerated culture employed which is completely sterilised by digestion with an equal volume of serum. No attempt is made to determine what reduction in number of living bacteria, and what subsequent increase occurs in the case of those tubes which are not completely sterilised.

It is claimed that by narrowing down the issue, as is here done, we escape from a fallacy which consistently arises in connection with estimations of bactericidal power arived at by a comparison of the

* *Vide* note on previous page.

results of bacterial enumerations carried out at a series of successive intervals upon one and the same mixture of serum and culture. The fallacy just referred to comes in in connection with the circumstance that all evidence of a bactericidal effect exerted will be obliterated if the intervals between the successive enumerations happen to be such as to allow of the covering up of losses due to the bactericidal action of the serum by a subsequent multiplication of the surviving micro-organisms.

A further point which has been kept in view in designing the above method, is the importance of obtaining a simple numerical expression for the bactericidal power of the blood.

Such a simple numerical expression is obtained by specifying the number of bacteria contained in 1 c.c. of the lowest dilution of the bacterial culture which is completely sterilised by digestion with an equal volume of serum.

While a convenient basis for the comparison of the bactericidal power of a series of different bloods is thus provided, it must be understood that the expression just referred to is nothing more than an arbitrary formula expressing the bactericidal effect of the serum brought into application in the form of a 50 per cent. solution.

If it is desired in any case to determine the bactericidal effect exerted by the serum in a practically undiluted condition, this can readily be achieved by making a graduated series of dilutions of the enumerated culture, using the serum itself as a diluent.

In concluding this section it will perhaps not be amiss to point out that the method of bactericidal estimation here described may be employed not only for determining the bactericidal power of the blood, but also for determining that of any chemical antiseptic.

Section II.—*Method of Measuring the Bactericidal Power of the Blood under Anaerobic Conditions.*

The method of measuring the bactericidal power of the blood under anaerobic conditions which is here to be described, is similar to the method described in the previous section, except in so far as the technique is modified with a view to excluding the air from contact with the blood.

Access of air is prevented by enveloping the blood in oil.

It is essential that this oil should be absolutely neutral, first, because the presence of fatty acid might affect the bactericidal power of the serum by diminishing its alkalinity and by precipitating its calcium salts, and, secondly, because an oil containing fatty acids is emulsified when it is brought in contact with serum, nutrient broth, and alkaline fluids generally. Such an emulsification would interfere with that sharp separation of the oil from the enclosed fluids which is absolutely

essential to the proper carrying out of the technique described below.

It will be well, therefore, to commence by describing the method adopted for the preparation of a fatty acid-free oil.

Method adopted for obtaining a Fatty acid-free Oil.

The method I have employed is a modification of that which was employed by the late Prof. E. Külz for obtaining a fatty acid-free oil for experiments in connection with pancreatic digestion.

The procedure will perhaps be most clearly described by detailing an actual experiment.

300 c.c. of a cheap variety of table oil (cotton oil ?)* was introduced into a litre flask along with 150 c.c. of half saturated barium hydrate solution. These fluids were digested together at 60° C. on a water bath for three hours, the contents of the flask being well shaken up at intervals.

After this time the contents had separated into three layers, an upper layer of more or less clear oil, a middle layer, about half an inch deep, of barium soaps, and a lower layer of barium hydrate solution.

A drop of the supernatant oil was now tested by shaking it up in a test tube with some 0·25 per cent. sodium carbonate solution. Indications of emulsification were quite absent.

The contents of the flask were now poured upon a wet filter. After the lapse of a few minutes, when the barium hydrate solution had filtered through, a clean dry beaker was placed under the funnel, and the whole filter stand was placed in a warm chamber. By next morning some 200 c.c. of clear oil were found in the beaker, the barium soaps having been left behind on the filter.

On shaking up the filtered oil with the sodium carbonate solution, it was found that this last showed a trace of turbidity. This turbidity was increased by breathing into the test tube and shaking up again.

In view of this, the whole volume of oil was now shaken up with distilled water, and a stream of carbonic acid gas was led through. The water and barium carbonate precipitate were then separated from the oil by filtration. When the oil thus purified was shaken up with the sodium carbonate solution, this last remained absolutely clear, the globules of oil remaining distinct and coming up promptly to the surface.

The fatty acid-free oil thus obtained is introduced into a stoppered

* The experiment described above can be perhaps even more easily carried out with any of the stable animal oils which are sold for lubricating fine machinery. Unstable oils, such as olive oil, are unsuitable, inasmuch as these last are broken up and converted into soaps when digested, as described above, with the barium hydrate solution.

bottle and is kept sheltered from light. Before it is employed for the purposes described below, it is sterilised by heating to 140° C. in a test-tube, and is each time re-tested by shaking up with the dilute sodium carbonate solution.

Procedure adopted for obtaining from the Finger a Sample of Blood without allowing this to come in contact with the External Air.

A receptacle for the blood is first provided by drawing out a test-tube to form such a "thimble" as is represented in fig. 5 (p. 66).

The thimble is filled in with sterilised fatty acid-free oil, and is covered in with a sterilised cover glass.

The ulnar aspect of a finger—preferably of the little finger of the left hand—is now sterilised by flaming alcohol. It is then punctured in two or three adjacent points by a fine spicule of glass. A clean handkerchief is wound round the digit, the tip of this last is immersed into the oil, and pressure is applied to the finger pulp. The blood as it emerges descends through the oil in the form of large globules.

When pressure on the pulp ceases to yield blood, the finger is momentarily removed from the oil, the handkerchief is loosened and re-applied, the finger is re-immersed into the oil, and pressure is again made on the finger pulp.

When a sufficiency of blood has been collected, a sterilised rubber test-tube cap is drawn over the thimble. This last is then placed in a hand-centrifuge, and the blood is, by a few turns of the handle, driven down to the lower narrow end of the tube.

After allowing an interval of 10 minutes to elapse, the centrifugalisation of the coagulated blood—the blood, it may be noted, invariably coagulates*—is taken in hand. The contents of the thimble will now arrange themselves into an upper layer of oil, a middle layer of clear serum, and a lower layer of blood corpuscles.

With a view to ensuring the asepticity of the further procedure, the serum may now with advantage be separated from the oil in the thimble, which has been exposed to some risk of aerial contamination.

Procedure for the Separation of the Serum from the Oil in the Thimble

The procedure is as follows :—

A series of three or four tubes, fig. 6, which are to function respectively as receiving and mixing tubes, are flamed, filled in with sterilised oil, horizontally inclined, and placed ready to hand. A capillary testing pipette, similar to that figured (fig. 4) and described

* (Added note.) The common text-book statement that coagulation is suspended when blood is collected under oil is, it may be presumed, based on experiments undertaken with oil containing free fatty acids. A decalcification of the blood might under such circumstances result.

n the first section of this paper, is fitted with a rubber teat; sterile oil is now aspirated into the pipette to replace the air in the capillary stem and the lower part of the bulb. When this replacement has been effected, the tip of the pipette is thrust down through the oil contained in the thimble into the layer of serum, and this last is aspirated into the pipette. When this has been accomplished, a little

FIG. 5.

1.



FIG. 6.

2.



of the covering oil is drawn into the capillary tube to seal the lower orifice of the pipette. The serum, thus shut off from contact with the air, is carried across into the first of the receiving tubes already spoken of, and is driven out into this under cover of the oil.

Another sterile testing pipette is now taken in hand, and the procedure is repeated in all its details, the serum being carried across from the first into a second receiving tube.

Procedure Adopted for Measuring off Equal Volumes of the Serum and Culture, and Mixing these without Contact with the Air.

The next step is to mix the serum with the graduated dilutions of the bacterial culture. These dilutions will have been prepared* by diluting the culture with sterile broth, which has been previously boiled up in order to remove the contained air.

The procedure by which the serum and the culture are mixed is essentially the same as the procedure employed for transferring the serum from one vessel to another. The capillary pipette is first filled in with a sufficiency of sterile oil, secondly with serum up to the mark on the stem. Thirdly, a globule of oil is drawn in.

Employing this globule of oil as a seal for preventing contact with the air—it will presently function as an index globule—the pipette is

* They may, if desired, be prepared under oil and from cultures anaerobically grown under a covering layer of oil.

carried across to the vessel which contains the culture, and is filled in with this last up to the mark on the capillary stem.

Mixture of the serum and culture can now be effected either (*a*) in the pipette itself, or—and I owe this suggestion to my colleague, Major W. B. Leishman, R.A.M.C.,—(*b*) in a mixing tube such as is shown in fig. 6.

In the former case, oil in sufficient quantity to seal the lower end of the column of fluid is aspirated into the pipette, after the volumes of serum and culture have been measured in in the manner described. The contents of this tube are then cautiously drawn upwards until the walls of the capillary stem fall away sufficiently to liberate the index globule of oil. This obstacle having been got rid of, a series of upwards and downwards displacements of the combined column of fluid will bring about the desired mixture.

In the case where a mixing tube is employed, the extremity of the capillary pipette is carried down to the floor of the mixing tube, and the contents are driven out under the covering seal of oil. They are then intermixed by alternately drawing them in and driving them out of the capillary stem, care being taken that the pipette is never emptied of oil. Lastly, the mixed fluids are carefully and completely reaspirated from the floor of the narrowed end of the tube, the inflow being in each case allowed to continue until a sufficient seal of oil has been carried in behind the mixture of serum and culture. As soon as this has taken place, the point of the pipette is withdrawn from the oil, and air is allowed to enter and occupy the lower third of the stem. Finally, the orifice is sealed in the flame.

In filling in a succession of capillary pipettes from one and the same mixing tube, it will, as consideration will show, be advantageous to begin with the highest dilution, and to follow on in order with the lower dilutions.

Procedure for determining the Bactericidal Effect exerted by the Serum in the absence of Air.

The process of filling in the bulb of the capillary pipette with sterile nutrient broth—a process which is, in the case of the ordinary aerobic procedure described in the previous section, undertaken as a first step in the filling in of the pipette—is, in the case of the anaerobic procedure, undertaken as the final procedure after the serum and the culture have been in contact for the desired period.

It is carried out in the following way:—

Sterile nutrient broth having been placed ready in a covered watch-glass, the capillary pipette which contains the highest dilution of the culture is taken in hand. A negative pressure having been established in its interior by fitting on a collapsed rubber teat, the lower portion

of the stem is passed through the flame of a peep-light in such a way as to heat it without allowing it to fuse and to collapse under the influence of the internal negative pressure. The sterilised extremity is now snapped off by plunging it while still hot into the nutrient broth. The inflow which takes place through the orifice thus provided is arrested by the pressure of the finger and thumb upon the teat as soon as the cultivation chamber is about two-thirds full.

The sealing up of the tube and the subsequent cultivations are carried out in exactly the same manner as in the case of the ordinary (aerobic) estimation described in Section I.

SECTION III.—*On the Bactericidal Effects produced by one and the same Human Blood* (a) *Drawn off and Tested by the Aerobic Procedure described in Section I; and* (b) *Drawn off and Tested by the Anaerobic Procedure described in Section II.*

In view of the fundamental theoretical importance which attaches to the assumption that the bactericidal power of the blood is acquired only after withdrawal from the organism, and, in particular, after the disintegration of the leucocytes under the influence of air and contact with the wall of unoiled or unparaffined receptacles, it seemed important to reinvestigate the question; I have therefore endeavoured to ascertain whether there is any constant and important difference between the bactericidal power of human blood (*a*) drawn off and tested by the aerobic procedure described in Section I; and (*b*) drawn off and tested by the anaerobic procedure described in Section II.

The results of this investigation are set forth below in tabular form, and it will be observed that while they are, of course, inconclusive on the wider question of the derivation of the bactericidal substances of the serum, they would seem definitely to show that neither contact with the external air, nor contact with ordinary glass surfaces, exerts any important influence on the bactericidal power exerted by human blood upon the typhoid bacillus and the cholera vibrio.

Table exhibiting the Bactericidal Effect produced by o

Dilutions in which the culture was employed.	1 vol. A. E. W.'s serum. 1 vol. of a typhoid culture, containing 156,000,000 T.B. per c.c.		1 vol. W. B. L.'s se 1 vol. of a typ culture, contai 156,000,000 T.B. c.c.	
	Aerobic procedure.	Anaerobic procedure.	Aerobic procedure.	Anaer proce
Undiluted.	—	—	—	—
2-fold dilution..	—	—	—	—
5 ,, ,,	Sterile	Sterile	Growth	Ster
10 ,, ,,	,,	,,	Sterile	,,
25 ,, ,,	,,	,,	,,	,,
50 ,, ,,	,,	,,	,,	,,
100 ,, ,,	,,	,,	,,	,,
1,000 ,, ,,	,,	,,	,,	,,
10,000 ,, ,,	,,	,,	,,	,,
100,000 ,, ,,	,,	,,	,,	,,

A. E. W. had been inoculated against typhoid; W. B. L., F. N.
The sera were in each case tested within 2—3 hours after the blo

e Bactericidal Effect produced by one and the same Serum (*a*) drawn off and tested by

. A. E. W.'s serum. ol. of a typhoid lture, containing 6,000,000 T.B. per c.		1 vol. W. B. L.'s serum. 1 vol. of a typhoid culture, containing 156,000,000 T.B. per c.c.		1 vol. F. N. W.'s serum. 1 vol. of a typhoid culture, containing 156,000,000 T.B. per c.c.		1 vol. A. E. W.'s serum. 1 vol. of a typhoid culture, containing 120,000,000 T.B. per c.c.	
robic cedure.	Anaerobic procedure.	Aerobic procedure.	Anaerobic procedure.	Aerobic procedure.	Anaerobic procedure.	Aerobic procedure.	Anaerobic procedure.
—	—	—	—	—	—	—	—
—	—	—	—	—	—	—	—
terile	Sterile	Growth	Sterile	Growth	Growth	Growth	Growth
,,	,,	Sterile	,,	,,	,,	,,	,,
,,	,,	,,	,,	,,	Sterile	,,	,,
,,	,,	,,	,,	,,	,,	Sterile	Sterile
,,	,,	,,	,,	,,	,,	Growth	,,
,,	,,	,,	,,	,,	,,	Sterile	,,
,,	,,	,,	,,	,,	,,	,,	,,
,,	,,	,,	,,	,,	,,	,,	,,

culated against typhoid; W. B. L., F. N. W., and A. B, were normal men; J. N. had recently convale
case tested within 2—3 hours after the blood had been withdrawn. The culture were in all cases aerob

ted by the Aerobic Procedure described in Section I; and (*b*) drawn off and tested

Capillary testing pipettes were filled in with —							
erum. phoid ining . per	1 vol. F. N. W.'s serum. 1 vol. of a typhoid culture, containing 150,000,000 T.B. per c.c.		1 vol. A. E. W.'s serum. 1 vol. of a typhoid culture, containing 100,000,000 T.B. per c.c.		1 vol. A. B.'s serum. 1 vol. of a typhoid culture, containing 220,000,000 T.B. per c.c.		1 vol. J. N 1 vol. o culture, 540,000, c.c.
robic dure.	Aerobic procedure.	Anaerobic procedure.	Aerobic procedure.	Anaerobic procedure.	Aerobic procedure.	Anaerobic procedure.	Aerobic procedure.
—	Growth	Growth	—	—	—	—	—
—	,,	,,	Growth.	Growth	—	—	Growth
wth	,,	,,	,,	,,	—	—	Sterile
,,	,,	,,	Sterile	,,	Growth	Growth	,,
,,	,,	,,	,,	,,	,,	,,	,,
rile	Sterile	Sterile	,,	,,	Sterile	Sterile	,,
,,	,,	,,	,,	,,	,,	,,	,,
,,	,,	,,	,,	,,	,,	,,	,,
,,	,,	,,	,,	,,	,,	,,	,,
,,	,,	,,	,,	,,	,,	,,	,,

y convalesced from typhoid.

ses aerobically grown 24-hours-old broth cultures. The serum was in each case allowed to act upon the

m (*a*) drawn off and tested by the Aerobic Procedure described in Section I; and (*b*) drawn

Capillary testing pipettes were filled in with –

erum. phoid ining . per	1 vol. A. E. W.'s serum. 1 vol. of a typhoid culture, containing 120,000,000 T.B. per c.c.		1 vol. F. N. W.'s serum. 1 vol. of a typhoid culture, containing 150,000,000 T.B. per c.c.		1 vol. A. E. W.'s serum. 1 vol. of a typhoid culture, containing 100,000,000 T.B. per c.c.		1 vol. A. B.'s seru 1 vol. of a ty culture, conta 220,000,000 T.B c.c.	
robic dure.	Aerobic procedure.	Anaerobic procedure.	Aerobic procedure.	Anaerobic procedure.	Aerobic procedure.	Anaerobic procedure.	Aerobic procedure.	Anae proc
—	—	—	Growth	Growth	—	—	—	
—	—	—	,,	,,	Growth.	Growth	—	
wth	Growth	Growth	,,	,,	,,	,,	—	
,	,,	,,	,,	,,	Sterile	,,	Growth	Gro
rile	,,	,,	,,	,,	,,	,,	,,	
,	Sterile	Sterile	Sterile	Sterile	,,	,,	Sterile	St
,	Growth	,,	,,	,,	,,	,,	,,	
,	Sterile	,,	,,	,,	,,	,,	,,	
,	,,	,,	,,	,,	,,	,,	,,	
,	,,	,,	,,	,,	,,	,,	,,	

l men; J. N. had recently convalesced from typhoid.

The culture were in all cases aerobically grown 24-hours-old broth cultures. The serum was in each case

) drawn off and tested by the Anaerobic Procedure described in Section II.

B.'s serum. f a typhoid containing ,000 T.B. per		1 vol. J. N.'s serum. 1 vol. of a typhoid culture, containing 540,000,000 T.B. per c.c.		1 vol. A. E. W.'s serum. 1 vol. of a cholera culture, containing 18,000,000 cholera vibrios per c.c.		1 vol. A. E. W.'s serum. 1 vol. of a cholera culture, containing 60,000,000 cholera vibrios per c.c.	
.	Anaerobic procedure.	Aerobic procedure.	Anaerobic procedure.	Aerobic procedure.	Anaerobic procedure.	Aerobic procedure.	Anaerobic procedure.
	—	—	—	Growth	Growth	Growth	Growth
	—	Growth	Growth	Sterile	Sterile	,,	,,
	—	Sterile	,,	,,	,,	,,	,,
	Growth	,,	,,	,,	,,	,,	,,
	,,	,,	,,	,,	,,	,,	,,
	Sterile	,,	,,	,,	,,	Sterile	Sterile
	,,	,,	,,	,,	,,	,,	,,
	,,	,,	,,	,,	,,	,,	,,
	,,	,,	,,	,,	,,	,,	,,
	,,	,,	,,	,,	,,	,,	,,

ach case allowed to act upon the culture for 18--24 hours at a temperature of 37° C.

"The Colour-physiology of Higher Crustacea." By FREDERICK KEEBLE, M.A., Reading College, Reading, and F. W. GAMBLE, D.Sc., Owens College, Manchester. Communicated by Professor S. J. HICKSON, F.R.S. Received July 16, 1902.

(Abstract.)

The following statement is a condensed summary of the results of a research into the form and physiology of the pigment-bearing organs (chromatophores) of certain Schizopod and Decapod Crustacea with especial reference to the effect of light on these organs and on these animals. The evidence for the statement will appear in a full and illustrated form in the "Philosophical Transactions." The Grant Committee of the Royal Society allotted £25 for this research.

A. *The Influence of Light.*

1. Under the influence of light the secretory activity of certain organs is modified: an acid substance appears periodically in the "liver" and muscle: the appearance and disappearance of acid substance in liver and muscle coincides broadly with nocturnal and diurnal colour-change.

2. In the progressive movements and orientations of the whole animal called forth by light, background is the most important factor: more powerful than change of light-intensity. By change of background, black to white, the direction of a light-induced movement may be reversed.

3. The response of the chromatophore-pigments to light is two-fold: direct; and indirect, through the mediation of the eye. The indirect response alone leads to an enduring redistribution of pigment.

4. The ultimate effect of monochromatic light on pigment-movement is the same as that of white light. As with the latter, so with monochromatic light, background—white (scattering), black (absorbing), mirror (reflecting)—determines the nature and extent of the pigment-movements. In describing an effect of light, that light must be considered in combination with its background. Neglect to do this must lead to erroneous conclusions.

5. "Reaction to background" is traceable to the eye, and is probably a consequence of an asymetrical distribution of retinal pigment brought about not by changes in the amount of light falling on the eye, so much as by changes in the way in which light falls on the eye.

B. *The* Rôle *of Pigments.*

6. The phenomena presented by the pigments are not exhaustively explained by any "protective" hypothesis.

The chromatophores are centres of metabolic activity, and from them a nocturnal translocation of a blue substance takes place. There is evidence that this blue substance is produced from, and at the expense of, the diurnal chromatophore-pigments. The blue substance passes from the chromatophore-centres, persists for a time in the body, and ultimately disappears.

C. *Morphology.*

7. The chromatophore-system of Mysidean Schizopods is built upon a common plan, of which the various genera and species present severally a constant modification. This system we call the primary chromatophore-system. To it the colour-pattern is due.

8. Decapod Crustacea possesses a primary and a secondary system of chromatophores. The primary system appears in the embryo, is completed in the "*Mysis*-stage," and persists throughout life, but takes no part in colour-pattern.

The secondary system arises in an early stage of development, increases in extent throughout life, and produces the colour-patterns of the adolescent and adult.

9. The chromatophores of the primary system are profusely branched, few in numbers, segmentally arranged and centralised; those of the secondary system are sparsely branched, numerous, irregularly arranged and decentralised.

D. *Histology.*

10. The chromatophores of Mysidæ are multicellular organs. Those of the neural group are developed from the epidermis. Losing their connection with the epidermis they acquire a close relation with the central nervous system. The distribution of the primary chromatophore-system follows that of the ganglionic parts of the nervous system.

11. The chromatophores of Decapods are plurinuclear connected structures: their distribution is not confined to the ganglionic parts of the nervous system.

E. *Taxonomy.*

12. The primary systems afford assistance in the determination of genera and species. By their aid, animals in early, as well as in late, stages of development may be diagnosed.

F. *Inheritance.*

13. The several adult colour-patterns of *Palæmon* and *Crangon* are constant, and develop directly. The evidence tends to prove that both secondary and primary chromatophore-systems are inherited.

14. The adult colour-pattern of *Hippolyte cranchii* is constant, but develops indirectly. The adolescent possesses a special colour-pattern developed in large measure in relation with the primary system of the zoea. Both persist though concealed by the independently developed adult pattern.

15. In *Hippolyte varians*, several adult colour-patterns occur. They develop indirectly. The primary system is the same in all.

In the adolescent, three distinct colour-patterns arise:—"barred," "liner," and "monochrome."

These may persist, becoming barred, liner, or monochrome adults.

Or either "barred" or "liner" may, by developing superficial or deep chromatophores, become a monochrome.

Or, by localised superficial developments either "barred" or "liner" may give rise to a "blotched" adult colour-form, under which the adolescent pattern is hidden.

The primary system is inherited: the adolescent colour-patterns are possibly inherited; but inheritance is immaterial since the final goal is reached by any adolescent road; that is, the adult colour-pattern of *Hippolyte varians*, is the result of environment.

"Observations on 'Flicker' in Binocular Vision." By C. S. SHERRINGTON, M.A., M.D., F.R.S. (Thompson-Yates Laboratory of Physiology, University College, Liverpool). Received July 30, 1902.

The connection between the physiological state and reactions of the two retinæ right and left is close in many respects; this is true particularly and peculiarly for their areas that are conjugate in binocular vision, that is, which receive corresponding images of objects perceived in the binocular field. The observations at basis of the following communication attempt to obtain further information regarding the nature of the tie between these retinal so-called "identical spots." A practical aim was to measure by the "flicker" method of photometry any difference of physiological luminosity existent between binocular and uniocular vision of a given illuminated object.

An object intermittently illuminated gives, if the frequency of intermission be sufficient, a perfectly steady sensation. The successive retino-cerebral reactions fuze into a continuous one as judged of by sensation. If the rapidity of intermission be less than the requisite, the sensation oscillates through lighter and darker phases. The transition from the oscillating to the steady sensation and *vice versâ* is sufficiently abrupt to form a transition point capable of fairly definite

fixation in time. It has been used in this enquiry as an index for noting the influence of the physiological state of a spot in one retina upon that of the "identical" spot in the other retina. A second index taken has been the visual "brightness" of the perceived image.

For the observations desired, it was considered important that extinction and re-illumination of the image occur *pari passu* in the two eyes, *i.e.*, with like speed, along a like direction in the retinal surface, and at "identical spots." It seemed also important to maintain perceptual singleness of the object seen, thus placing the "identical spots" in the most favourable condition for their co-operative identity. For this reason a steady and considerable degree of "convergence" of the visual axes was made one condition of the observations. The degree of focal accommodation of the eyes was arranged to correspond with the amount of convergence. Variations in the aperture of the pupil were excluded by wearing small artificial pupils.

The illuminated object was arranged as follows. A small double sheet of thick "milk" glass was illuminated by a candle-shaped, single-loop, incandescent electric lamp, with frosted glass front. The lamp was fed at rather above its intended voltage by accumulators unused during the experiment for any other purpose than the lighting of the lamp. The lamp was set in the axis of a rotating cylinder. In this latter were openings of appropriate size, different according to the different requirements of the observation. The lamp was not fixed to the cylindrical screen. The milk glass was set close inside this screen. Outside the moving cylindrical screen was a fixed cylindrical screen concentric with the revolving one. In the fixed cylindrical screen four circular holes were arranged so that two were centred on the same horizontal line, and of the other two, one was centred as far above the left-hand hole of the previous pair as the second was below the right-hand member of the pair. These holes were viewed from a distance such that when the line of the visual direction of the left eye passed through the centre of the lower left hole it met at the axis of the cylindrical lantern, the line of visual direction of the right eye passing through the centre of the upper right-hand hole. A blackened vertical screen cut off from the left eye all view of the right-hand holes, and from the right eye all view of the left-hand holes. A pair of weak prisms with base-apex line vertical sufficed to bring the images of the right-hand and left-hand holes to the same levels. The images then immediately fused under convergence. Cross horizontal and vertical wires across the component holes served to certify binocular or monocular vision to the observer. When the four holes were all allowed to contribute images they were seen by the observer as two small evenly-lighted discs, one vertically above the other, with a delicate cross-line on each. Any one of the holes could be separately screened out of vision.

The spindle of the revolving cylindrical screen carried a step-pulley. From this a cord ran to a step-pulley fixed on the spindle of a small electromotor. The speed of running of this motor was controlled by a set of coil resistances, which formed a coarse adjustment, and by a fluid resistance in a trough 1 metre long, with a sliding electrode; this formed a fine adjustment. The speed of rotation of the cylindrical screen was recorded by marking the completion of each revolution of the spindle carrying the screen by an electro-magnetic signal writing on a travelling blackened surface. On the same surface the time was recorded by a writing clock marking fifths of seconds.

The observations required an operator to manage speed of motor, registration of time and revolutions, &c., and an observer who, seated in a dark compartment, gave his attention to the watching of the illuminated discs.

The shutters of the rotating cylindrical screen could be arranged so that the illumination of a retinal spot in one eye could be varied in time and intensity synchronously or asynchronously with that of the conjugate spot of the other eye. Of various combinations examined, the following may be cited as outlining the evidence obtained. The discs may be referred to as disc A and disc B.

Experiment 1.—When the disc A represents right retinal stimulation alone (*i.e.*, without left), the conjugate spot of the left eye remaining dark; and when the disc B represents right retinal stimulation with coincident synchronous (*i.e.*, of synchronous phase) intermittent stimulation of the conjugate spot of the left retina; the frequency, duration, and intensity of the light periods being alike at all the holes.

Then, steady sensation is obtained from disc A at frequencies of intermission lower than those required for giving steady sensation from disc B.

At speeds sufficient to give steady sensation at both discs, the disc B does not obviously differ in brightness from the disc A.

Experiment 2.—When disc A represents as before uniocular stimulation only,

And when disc B represents intermittent right retinal stimulation, together with a stimulation yielding steady sensation from the conjugate field of left retina.

And when the steady sensation from left retina corresponds with an intensity of light stimulus half that of the intermittent stimulus employed for the right eye, then the rate of intermission required for obtaining steady sensation from disc A is higher than that required for obtaining it from disc B.

At speeds sufficient to give steady sensation for both discs, the disc B does not obviously differ in brightness from the disc A.

Experiment 3.—The disc A representing uniocular vision as before.

When disc B represents intermittent right retinal stimulation with,

in same way as in 2, continuous steady sensation from the conjugate left retinal spot,

And when the intensity of that continuous sensation from left retina corresponds with an intensity of light stimulus more than half that of each stimulus employed in repetition for the conjugate of the right eye,

Then the rate of intermission required for obtaining steady sensation from disc A is higher than that required for obtaining it from the disc B.

Of the two discs, seen under speeds sufficient to give steady sensation from both, disc B appears the brighter.

Experiment 4.—The disc A representing uniocular vision as before.

When the disc B represents intermittent right retinal stimulation, together with, in the same way as in 2 and 3, steady sensation from the conjugate left retinal spot,

And when the intensity of that steady sensation from left retina corresponds with that due to a light stimulus of less than half the intensity of each member of the series of repeated stimuli employed for the conjugate spot of the right eye,

Then the rate of intermission required for obtaining steady sensation from disc A is higher than that required for obtaining it from the disc B.

And of the two discs, both seen under speeds sufficient to yield steady sensation, disc A appears the brighter, unless the field offered to the left eye is given by closing that eye or otherwise screening with a homogeneous darkness.

Experiment 5.—The disc A representing uniocular vision as before,

And when the disc B represents intermittent right retinal stimulation, the intervals of intermission exactly corresponding with periods of illumination of the conjugate spot of the left retina, and the intermittent stimuli being equal in intensity and duration for both right and left conjugate spots;

Then the rate of intermission required for obtaining steady sensation from disc A is higher than that required for obtaining it from disc B.

Of the two discs, both seen under speeds of intermission sufficient to yield steady sensation, the disc B appears to be of a brightness not obviously different from that of disc A.

Experiment 6.—With disc A as before.

When disc B represents intermittent right retinal stimulation, similar in every way to that applied to the conjugate of the left retina, except that its phases of light and shade precede or succeed those applied to the other retina by an interval of half a phase length,

Then the rate of intermission required for obtaining steady sensa-

tion from disc B does not appreciably differ from that required to obtain it from disc A.

Of the two discs, at speed sufficient to yield steady sensation, disc B does not obviousiy differ in brightness from disc A.

Experiment 7.—When disc A represents the binocular combination described for disk B, Experiment 1, and the disk B is as described in Experiment 6, the rate of intermission required to obtain steady sensation from A is higher than that required for obtaining it from B, but at speeds sufficient to yield steady sensation from both discs the two discs appear to be of equal brightness.

Experiment 8.—With disc A, as in Experiment 7, and disc B, as described in Experiment 6, the frequency of repetition of stimulus required to yield steady sensation from disc A is slightly higher than that required to obtain it from disc B, but at speeds sufficient to yield steady sensation from both discs the two discs appear of equal brightness.

Experiment 9.—With disc A, representing the binocular combination described for disc B in Experiment 5, and with disc B, as described in Experiment 6, the frequence of repetition of stimulus required to yield steady sensation at disc A is slightly lower than that required to yield it at disc B; but at speeds sufficient to yield steady sensation from both discs, the two discs appear of equal brightness.

The observations show (i) that Talbot's law, unimpeachable (over a wide range of ordinary luminosities) for the single eye, is not applicable to combined binocular vision, that is, that if the two eyes functioning together in binocular vision are considered as functionally to combine to a single organ, Talbot's law does not hold good for that organ as it does for the single eye, right or left. (ii) That increase of luminosity of an intermitting image does not always necessitate increase of rate of frequency to extinguish its flicker; and conversely, They also show that the "Fechner paradox" regarding binocular luminosity makes itself apparent under "flicker" examination as well as under "brightness" estimation.

It seems that the physiological sum of two luminosities perceived through conjugate retinal areas is of a value intermediate between the individual values of the two component luminosities.

Among experimental difficulties incident to the experiments may be mentioned the increased perception of flicker under paracentral as com pared with central locus of stimulus on the retina, as noted by Exner and by Charpentier. Interesting experimental difficulties were also occasioned by the reciprocal and often antagonistic influences exerted by one retina upon another in ways studied and described recently by Dr. W. McDougall.*

It was further observed that binocular colour mixture did not seem

* 'Mind,' 1901.

to be either rendered easier or impeded when the components were applied by alternating right and left stimuli as compared with the method of applying them by coincident right and left stimuli.

"On the Influence of the Prolonged Action of the Temperature of Liquid Air on Micro-organisms, and on the Effect of Mechanical Trituration at the Temperature of Liquid Air on Photogenic Bacteria." By ALLAN MACFADYEN, M.D. Communicated by Professor JAMES DEWAR, F.R.S. Received August 2, 1902.

In previous communications it was shown that an exposure for twenty hours and for a period of seven days to the temperature of liquid air (about -190° C.) had no effect on the vitality of micro-organisms, whilst an exposure of ten hours to a temperature as low as that of liquid hydrogen (about -252° C.) was likewise without an appreciable effect.*

Further experiments have since been made in which the influence of the prolonged action of the temperature of liquid air on organisms was tested for a period of six months.

The bacteria employed were non-sporing forms, viz., *B. typhosus*, *B. coli communis*, and *Staphylococcus pyogenes aureus*, along with a Saccharomyces.

The bacteria were directly immersed in the liquid air, either on cotton-wool swabs enclosed in a perforated metal case, or on small loops of platinum wire. The yeast was washed and pressed, then wrapped up in rice paper, and directly exposed.

Samples were removed and tested at various intervals up to six months. In no case could any impairment of the vitality of the organisms be detected. The fresh growths obtained were normal in every respect, and the functional activities of the organisms were unaffected. The typhoid bacillus retained its pathogenic properties, and responded typically to the agglutination test; the colon bacillus exhibited its normal properties; the *Staphylococcus aureus* produced pigment on solid and an active hæmolysin in fluid media, whilst the yeast exhibited its fermentative power unimpaired.

The above experiments show that a prolonged exposure of six months to a temperature of about -190° C. has no appreciable effect on the vitality of micro-organisms. To judge by the results, there appeared no reason to doubt that the experiment might have been successfully prolonged for a still longer period.

* 'Roy. Soc. Proc.,' February 1, 1900; *ibid.*, April 5, 1900; *ibid.*, May 31, 1900.

The ordinary manifestations of life cease at zero, but at – 190° C. we have reason to suppose that intracellular metabolism must in addition practically cease—as a result of the withdrawal of two of its cardinal physical conditions, viz., heat and moisture. It is difficult to form a conception of living matter under this new condition, which is neither life nor death, or to select a term which will accurately describe it.

In previous experiments it was found that the photogenic bacteria preserved their normal luminous properties after exposure to the temperature of liquid air. On re-thawing, a rapid renewal of the photogenic properties of the cells occurred. The light is apparently produced by a chemical process of intracellular oxidation. The feasibility of triturating micro-organisms at the temperature of liquid air has now been experimentally established in the case of the typhoid bacillus and other bacteria.* The effect of such mechanical trituration at the temperature of liquid air on the luminous properties of the photogenic bacteria has now been tested. The experiments have shown that the effect of such a trituration is to abolish the luminosity of the cells in question.

This points to the luminosity being essentially a function of the living cell, and dependent for its production on the intact organisation of the cell.

I am indebted to Professor Dewar for valuable suggestions, and to Mr. Sydney Rowland and Mr. J. E. Barnard for their assistance in the experiments, which were carried out at the Jenner Institute of Preventive Medicine.

"An Intracellular Toxin of the Typhoid Bacillus." By Allan Macfadyen, M.D., and Sydney Rowland, M.A. Communicated by Lord Lister, F.R.S. Received August 14, 1902.

The existence of a specific toxin produced by the typhoid bacillus has hitherto not been demonstrated, although it has been assumed by analogy with other organisms and by reasoning from the clinical course of the disease.

Such a poison must either be intracellular or extracellular.

That it does not exist in filtered cultures of the organism is the common experience of bacteriologists. Its absence from such cultures might be due, however, to unsuitability of the soil used for growing the organism.

* "The Intracellular Constituents of the Typhoid Bacillus," Allan Macfadyen and Sydney Rowland, 'Centralblatt f. Bakteriologie,' vol. 30, 1901, No. 20.

Accordingly the first step in the search for the body in question consisted in substituting for the usual broth and peptone media, culture fluids approaching more nearly in constitution the natural body soils which clinically support the growth of the bacillus. For this purpose, the organism was grown on the actual intracellular juices of the following organs and tissues obtained in a fresh condition from the ox or calf:—

Intestinal mucous membrane, mesenteric lymphatic glands and spleen. In each case the intracellular juice was brought to the requisite degree of alkalinity and used as a culture soil under the following conditions:—

1. Aerobically. 2. Anaerobically. 3. With addition of normal human serum. 4. After heating to 55° C. for 20 minutes.

After from 4 to 6 weeks' growth the organisms were filtered off and the filtrate tested for toxicity in guinea-pigs. With the possible exception of one spleen juice, none of the fluids thus obtained exhibited any acute toxic power. It thus became necessary to search within the typhoid organism itself for the missing toxin. For this purpose the organisms were grown on ordinary beef broth agar, and after careful washing with distilled water were disintegrated in a mechanical contrivance at the temperature of liquid air (−180° C.) This course was taken to satisfy the conditions that—(1) No chemical change should take place during the disintegration, and (2) The organisms could be disintegrated alone, without the addition of any triturating substance, the necessary subsequent removal of which might vitiate the composition of the resulting mass. If such a disintegrated mass be freed from whole bacilli (if present) and from other suspended insoluble particles by centrifugalisation, an opalescent fluid results, which on inoculation into animals in small doses invariably proves toxic or fatal. It is therefore concluded that the typhoid bacillus contains within itself an intracellular toxin.

The typhoid cell juices obtained by the above method are being examined for immunising and other properties at the Jenner Institute of Preventive Medicine, where the above investigations have been conducted.

"The Fracture of Metals under repeated Alternations of Stress." By J. A. EWING, LL.D., F.R.S., Professor of Mechanism and Applied Mechanics in the University of Cambridge, and J. C. W. HUMFREY, B.A., St. John's College, Cambridge, 1851 Exhibition Research Scholar, University College, Liverpool. Received August 11, 1902.

(Abstract.)

The paper describes an investigation by means of the microscope of the process by which iron becomes "fatigued" and breaks down when subjected to repeated reversals of stress, as in Wöhler's experiments. It is shown that although the greatest stress is much within the limit of elasticity (as determined by the proportionality of strain to stress in an ordinary tensile test), it produces rupture after many reversals. The first visible effect is the production of slip-bands here and there on individual crystals. These gradually become more numerous: they also become accentuated and broadened and their edges turn rough and burred, apparently as a result of grinding of one surface on the other over the plane in which slip has occurred. At a later stage certain of the slip-bands develop into cracks, whose existence can be demonstrated by repolishing the specimen, when the slip bands which have not opened into cracks are obliterated, but the cracks remain visible as actual fissures. As the process of reversals goes on, the cracks spread from crystal to crystal, and fracture ensues. In the particular material dealt with, Swedish iron, having an elastic limit in tension of about 13 tons per square inch and a breaking strength of 23·6 tons per square inch, it was found that a stress not exceeding 9 tons per square inch, when reversed some millions of times, was sufficient to develop cracks and to bring about the fracture of the piece. Stresses of 8 and even 7 tons per square inch were found to develop slip bands which would probably turn into cracks under a sufficient number of reversals. The paper is illustrated by micro-photographs taken at various stages of the destructive process.

"On Changes in Elastic Properties produced by the sudden Cooling or 'Quenching' of Metals." By JAMES MUIR, B.A., D.Sc., late 1851 Exhibition Science Research Scholar. Communicated by Professor EWING, F.R.S. Received August 11, 1902.

(Being part of a Thesis submitted for the degree of Doctor of Science, Glasgow University.)

It is well known that when steel is quenched from a red heat, its elastic properties suffer a profound change, the material becoming extremely hard and brittle. It is also known that quenched steel, when tested under tension, exhibits no distinct yield-point, Hooke's law is departed from quite gradually until abrupt fracture occurs at a high stress. The effect produced on copper by quenching has been considered, at least in some respects, the reverse of that produced in steel. The experiments to be described in this paper, however, show that with mild steel, soft iron, copper, zinc, aluminium, brass, and so probably with all metals, quenching from high temperatures produces effects which are analogous to one another; in all cases there is a marked loss of elasticity produced by quenching, low loads producing appreciable permanent extensions or "sets."

The method of experimenting need not be described in detail here, as it was identical with that described in the paper by the present author on "The Tempering of Iron hardened by Overstrain."* The new 5-ton testing machine of the Cambridge Engineering Laboratory was however employed for many of the experiments in preference to the large 50-ton gun machine previously used. Small strains of extension and of compression were measured by instruments of Professor Ewing's design—extensions by means of the 4-inch extensometer illustrated on p. 2, 'Phil. Trans.,' A, 1902, compressional strains by the instrument illustrated on p. 79 of Professor Ewing's book on "The Strength of Materials." The heating of the specimens was obtained by means of the gas furnace used in the earlier experiments on tempering after overstrain, temperatures being measured by a Callendar's direct-reading platinum resistance pyrometer. The hot specimens were "quenched" by plunging them vertically into a large tank of cold water.

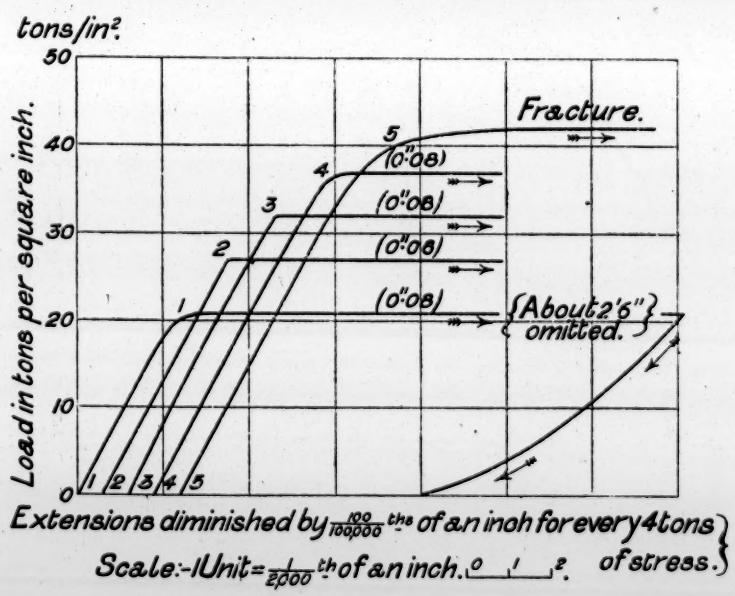
The results to be recorded in this paper may be gathered from an examination of the accompanying series of diagrams. The diagrams give with one exception (Diagram 4) the results of tension tests, and it need only be remarked that all the stress-strain curves have been "sheared back" in the manner suggested by Professor Ewing, and

* 'Phil. Trans.,' A, 1902, p. 1.

fully described in a paper by the present author "On the Recovery of Iron from Overstrain."* The amount by which the curves have been sheared back is marked at the foot of each diagram. Thus, in Diagram No. 1, $\frac{100}{100000}$ths of an inch have been deducted from the extension of the 4-inch length for every 4 tons of stress. For example, the extensometer readings for stresses of 4, 8, and 12 tons per square inch were 120, 240, and 360 respectively; the numbers actually plotted were 20, 40, and 60. The origin for the measurement of extensions has been displaced for the various curves of each diagram in order to avoid a confusion of the curves.

Diagram No. 1 shows the elastic properties of an annealed specimen of mild steel. The specimen was subjected to a series of tension tests, the load in each test being carried just to a yield-point. Recovery from the overstrains produced by the passing of the successive yield-points was effected by heating the specimen to temperatures of from

DIAGRAM No. 1.—(Mild steel-annealed.)



Diameter of specimen = 0"·331.
Length under test = 4"·00.
Fracture occurred at 40 tons per square inch original area. Extension (including all yield-points) = 0"·38 on 4 inches.

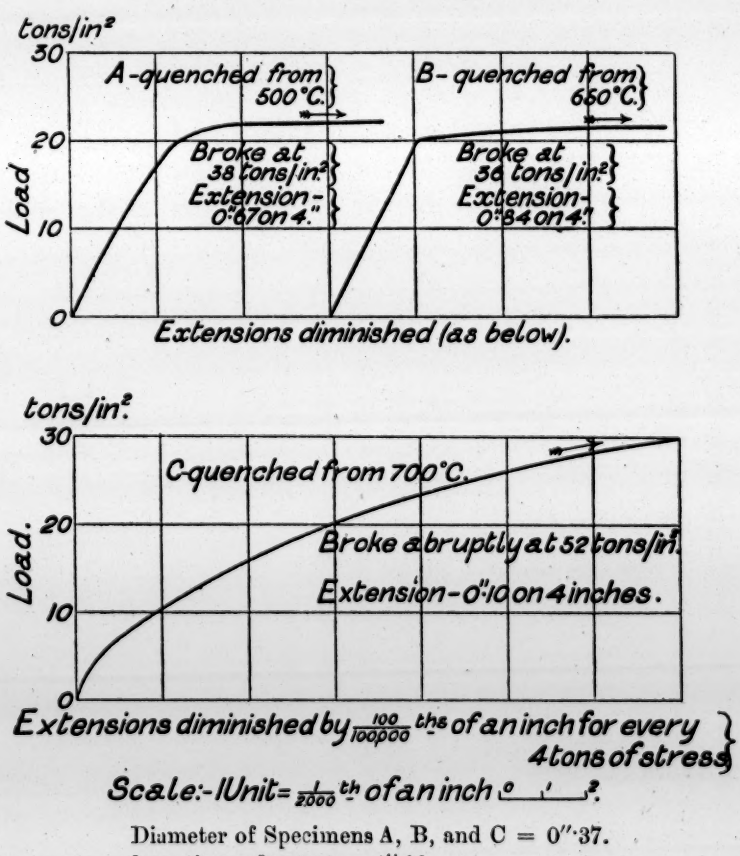
200° to 250° C. The specimen broke at the fifth yield-point, the breaking stress being 42 tons per square inch, or about 40 tons per square inch taking the original area of the specimen. After the passing of each yield-point the diameter of the specimen was of course

* 'Phil. Trans.,' A, vol. 193, 1899.

slightly reduced; this was allowed for in the succeeding loadings, the load being always applied in tons per square inch of actual section.

Diagram No. 2 shows the elastic properties of the same steel after it had been heated to 500°, to 650°, and to 700° C., and quenched in water at about 15° C. Each of the specimens employed was broken by a single continuous loading. Curves A and B show that quenching from 500° and from 650° C. had little effect on the elastic properties of the steel. The specimen from which Curve B was obtained had been more thoroughly annealed before quenching than Specimen A, and this may account for the lower breaking load and greater ultimate extension obtained with Specimen B, although all the specimens employed were primarily annealed. Curve C shows that a marked change was produced in elastic properties by quenching from 700° C.

DIAGRAM No. 2.—(Mild steel-quenched.)



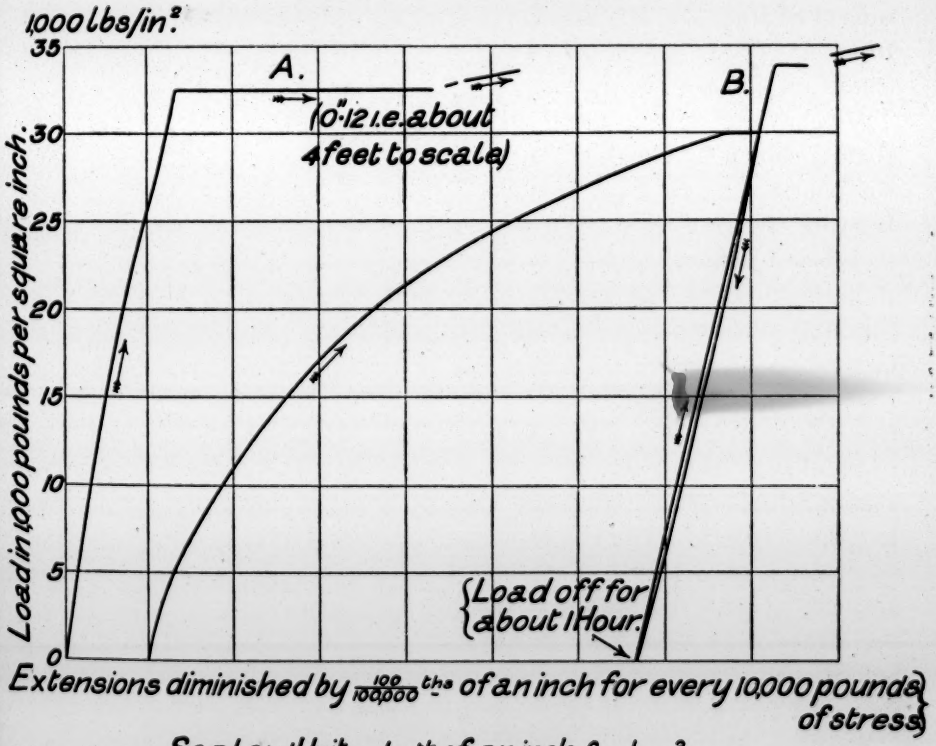
Diameter of Specimens A, B, and C = 0″·37.
Lengths under test = 4″·00.

The material after quenching showed no range of elasticity. Hooke's law was departed from gradually from the lowest loads till ultimately fracture occurred abruptly at the high stress of 52 tons per square inch. The ultimate extension was only 0″·10 on 4 inches.

Diagram No. 3 shows the effect produced on the elastic properties of Lowmoor iron by quenching from 700° C. Curve A was obtained from an annealed specimen of the material. A very clearly defined yield-point is shown at the stress of 33,000 lbs. per square inch. After the yielding at the yield-point had spread throughout the specimen, the load was steadily increased until fracture occurred at the stress of 50,300 lbs. per square inch. The extension produced was 0″·95 on 4 inches, neglecting all the local extension which occurred at the point of fracture, or 1″·23 including the local extension.

Curve B, which was obtained from a specimen which had been quenched from 700° C., clearly shows the loss of elasticity produced by quenching. A curious recovery effect was noticed in this test. The load was applied until a stress of 30,000 lbs. was attained, and

DIAGRAM No. 3.—(Lowmoor Iron.)



Diameter of specimens A and B = 0″·44.
Length under test = 4″·00.

Specimen A.—Annealed at 750° C. Broke at 50,300 lbs. per square inch.
Extension 0″·95 omitting, or 1″·23 including, local extension.

Specimen B.—Heated to 740° C., slowly cooled to 700° C., and then quenched in cold water. Broke at 61,300 lbs. per square inch.
Extension 0″·61 omitting, or 0″·88 including, local extension.

was then removed. The contraction which occurred on the removal of the load was almost perfectly elastic. Had the load been immediately replaced, the material would have shown perfect elasticity up to the stress of 30,000 lbs., but immediately the load was increased beyond this amount larger yielding would have occurred, and a smooth continuation of Curve B would have been obtained. The specimen was, however, allowed to rest for about an hour before the load was replaced and increased. This rest proved to have a comparatively large effect, the material showing very perfect elasticity up to the stress of 34,000 lbs. per square inch. At this stress a partial yield-point was exhibited (represented by about 6 units of extension on the diagram), and on increasing the load gradual extension was produced, until ultimately fracture occurred at the high stress of 61,300 lbs. per square inch. The ultimate extension is marked at the foot of the diagram, and was less than that obtained with annealed material.

It may be recorded that another specimen of this Lowmoor iron was quenched from 700° C., but, before testing, this specimen was re-heated to about 200° C. in order to see if any appreciable return to the elastic condition illustrated by Curve A, Diagram 3, would be obtained. The behaviour of the specimen was more nearly elastic for low loads than is shown by Curve B, but all the main features of Curve B were corroborated; gradual departure from Hooke's law was obtained until fracture occurred at 61,500 lbs. per square inch, the extension being 0″·54 on 4 inches omitting, or 0″·9 including, local extension.

Before leaving the consideration of iron and steel, the effect produced by quenching iron, as illustrated by compression tests, may next be considered.

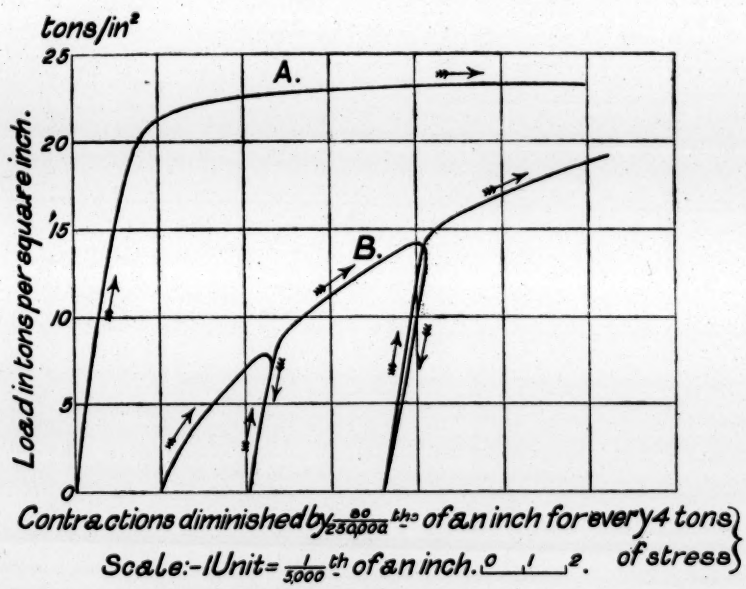
Diagram No. 4 shows by a comparison of two compression curves the change in elastic properties produced by quenching mild steel from a red heat. Specimen A, was a short annealed block of mild or semi-mild steel; the diameter of the specimen was 1″·156 and its length $1\frac{7}{8}$ inches. The compression instrument employed enabled the contraction on a length of $1\frac{1}{4}$ inches to be measured to the $\frac{1}{250000}$ of an inch. Curve A shows that the annealed material was elastic up to the stress of 21 tons, but 23 tons per square inch had to be applied before really large yielding occurred. A tension test of this material showed a well-defined yield-point at $22\frac{1}{2}$ tons per square inch.

Specimen B was exactly similar to Specimen A, but the material in this case instead of being annealed was heated to redness and quenched in cold water. Curve B shows the marked loss of electricity produced by the quenching. The rounding of Curve B at the two points where the load was removed is probably to be accounted for by experimental errors of the nature of back-lash in the testing machine or compression instrument.

Diagram No. 5 illustrates the results obtained by experiments with

copper rods. Curve A of that diagram was plotted from a tensile test made with the material in the condition as supplied. Curve B shows the elastic properties of the material after it had been heated to 630° C., and allowed to cool slowly, while Curves C_1, C_2, and C_3 show the effect produced by quenching the copper from 500° C., from 550° C., and finally from 600° C. Specimen B showed more perfect elastic behaviour for low loads than Specimen A, but large extension is shown by Curve B to have occurred earlier with the annealed material. Specimen C was first heated to 500° C., and quenched in cold water.

DIAGRAM No. 4.—(Mild steel under compression.)



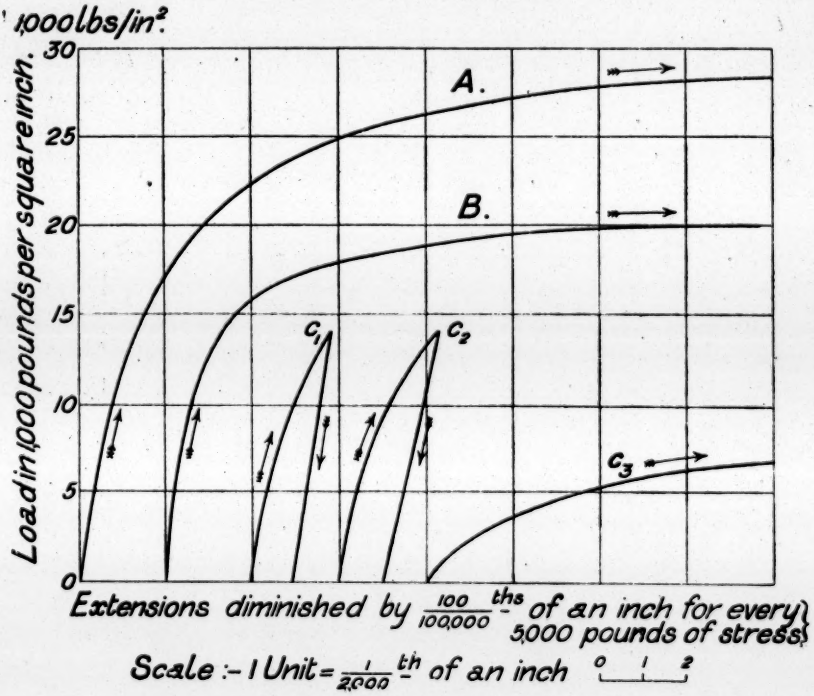
Specimen A.—Annealed.
„ B.—Quenched from a red heat.

Diameter of specimens = 1·16 inches.
Total length = 1⅞ inches.
Contraction measured on 1¼ inches.

Curve C_1 was then obtained by applying and removing a load of 15,000 lbs. per square inch. The specimen was next quenched from 550° C., and Curve C_2 shows the slightly greater loss of elasticity which was thus produced. Curve C_3 shows the large effect caused by quenching the Specimen from 600° C. The breaking stresses obtained with the three specimens were 35,300, 33,500, and 32,300 lbs. per square inch of original area. These stresses were equivalent to 41,600, 40,800, and 44,200 lbs. per square inch, when allowance was made for the diminutions in area due to the large extensions of the specimens before fracture. These corrections were made by

calculating the reduced areas from the extensions obtained (omitting the local extensions at points of fracture) and neglecting the small changes in density which are known to be produced by stretching. Copper thus resembles iron and steel in having its breaking stress

DIAGRAM No. 5.—(Copper.)



Diameter of specimens = 0″·37. Length under test = 4″·00.

Specimen A.—Copper as supplied. Broke at 35,300 lbs. per square inch original area, or after an actual stress of 41,600 lbs. per square inch had been applied to the bar. Extension on 4 inches, 0″·74 omitting, or 1″·04 including, local extension.

Specimen B.—Heated to 630° C. and slowly cooled. Broke at 33,500 lbs. per square inch original area, or 40,800 lbs. per square inch actual stress. Extension on 4 inches, 0″·87 omitting, or 1″·25 including, local extension.

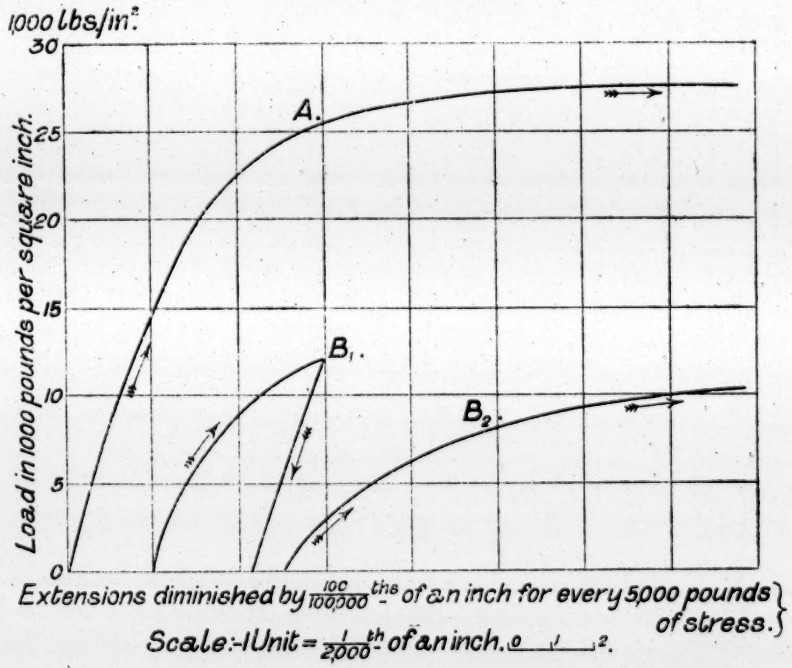
Specimen C.—Quenched from 500°, 550°, and then from 600° C. Broke at 32,300 lbs. per square inch original area, or 44,200 lbs. per square inch actual stress. Extension on 4 inches, 1″·50 omitting, or 1″·98 including, local extension.

increased by quenching, but differs from iron and steel in giving a greater extension before fracture when in the quenched condition. The abrupt yield-point which is so striking a feature in the testing of annealed iron and steel, is not exhibited with copper. The ultimate extensions obtained with the three specimens of copper tested were 0″·74 on 4 inches with A, 0″·87 with B, and 1″·50 with C, omitting

the local extensions at the points of fracture, or 1″·04, 1″·25, and 1″·98 respectively including the local extensions.

Diagrams Nos. 6 *and* 7 may now be given without comment. They illustrate tests made with brass and aluminium, and it is shown in both cases that there is a loss of elasticity produced by quenching. When the quenched material has been once loaded it is brought approximately into the elastic condition, so that from a removal and reapplication of load a straight stress-strain curve is obtained.

DIAGRAM No. 6.—(Brass.)



Diameter of specimens = 0″·44. Length under test = 4″·000.

Specimen A.—Brass as supplied. Broke 4 times in machine grips at about 48,500 lbs. per square inch. Extension from 0″·87 on 4 inches after the first break to 1″·68 after the fourth.

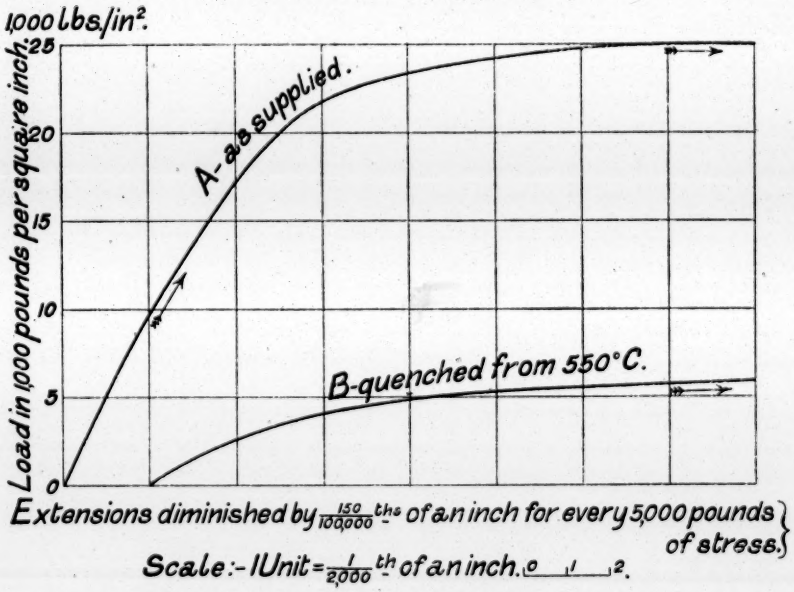
Specimen B.—Brass quenched from 600° C. (B_1) and from 700° (B_2). Broke at 42,500 lbs. per square inch after two breaks in the machine grips at slightly lower stresses. Extension, 1″·33 on 4 inches.

Three specimens of zinc, of diameter 0″·40, were also tested. The first—in the condition as supplied—broke at 21,000 lbs. per square inch, the ultimate extension being 2″·04 on 4 inches. There was great local extension, the specimen being drawn to a very narrow neck before fracture. The second specimen was quenched from 350° C. It yielded rather more for the lower loads than the first specimen, and broke at 20,500 lbs. per square inch with an extension of 0″·41 on 4 inches. The fracture was quite abrupt, so that there was little or no

local extension. The third specimen was heated to 350° C., and allowed to cool in the air. The behaviour of this specimen was very similar to that of the quenched specimen. Rather less yielding was obtained at low stresses, fracture occurred at 21,400 lbs. per square inch, the ultimate extension was 0″·68 on 4 inches. The fracture was abrupt, so that there was practically no local extension.

Two specimens of cast tin were also tested, but owing to the low melting point of tin quenching from 200° C. could only be tried. The

DIAGRAM No. 7.—(Aluminium.)



Diameter of specimens = 0″·35. Length tested = 4″·00.

Specimen A.—As supplied. Broke four times in machine grips at 28,500 lbs. per square inch. Extension 0″·14 on 4 inches.

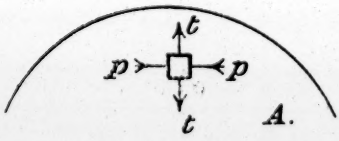
Specimen B.—Quenched from 550° C. Broke in the machine grips at 23,200 lbs. per square inch. Extension 0″·9 on 4 inches.

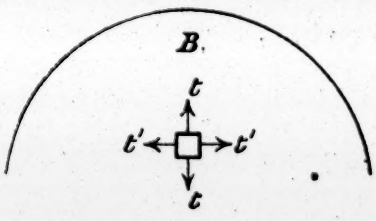
quenched specimen showed rather greater extension at the lower stresses; both specimens broke at 5250 lbs. per square inch; the extensions were 0″·5 and 0″·8 on the 4-inch lengths, but local extension occurred in several places before fracture.

In conclusion, it is proposed to consider how far the effects produced by quenching described above may be accounted for by the stresses set up in the material by the sudden cooling and consequent contraction, the material, after quenching, being no longer in what has been termed its "state of ease."

When a long cylindrical rod cools, the cooling takes place radially, and the end effects may be neglected. Taking any cross-section of the rod, the outside ring will cool first and assume its elastic state; the

interior will then contract, and exert a radial pull on the outside solidified layer. This will put the material into a state of circumferential compression. If the tangential direction be called the direction of X, the radial direction that of Y, so that the rod considered lies along the axis of Z, then the material in the outside layer of the quenched rod of metal is subjected to a compressional stress in the X direction. If a layer of material be considered at some distance from the outside it will be found to be subjected not only to a compression in the X direction, but also to a tension in the Y direction. For the outside solidified layers are able to resist to some extent the radial pull due to contraction. A particle of material at a point such as A will thus be subjected to stresses p and t in the manner illustrated in the sketch. Going nearer the centre of the bar, the pull due to contraction of the hot material may be more than balanced by the outward radial pull due to the solidified material which has settled down under radial tension, so there may be a resultant outward pull all round the layer considered, and a particle such as B will be subjected to a circum-





ferential pull, t', as well as a radial pull, t. There will be, of course, a gradual transition from material in the one condition to material in the other.

Further, the stresses induced by sudden cooling will probably be severe enough to overstrain many layers of material, and, except in the case of portions which have been overstrained when quite cool, recovery from overstrain will be effected, so that the material will be left in an elastic condition, hardened as regards the stresses in question, and not in the semi-plastic state typical of material which has been recently subjected to overstrain.

Now it is well known that when metals are deformed they alter very little in volume, almost the whole strain is one due to change of shape. It is only necessary then to consider the shear stresses applied by the systems of stresses illustrated above at A and B. A pull (t or t') is equivalent to a hydrostatic tension ($\frac{1}{3}t$ or $\frac{1}{3}t'$) and two shear stresses in definite directions; a push (p) gives rise to a hydrostatic pressure ($\frac{1}{3}p$) and two shear stresses. A circumferential pressure (p case A) gives rise to the following two shears:—

1_A, giving contraction in direction of X and extension in direction of Y,
2_A „ „ „ X „ „ Z.

A radial tension (*t* cases A and B) gives rise to the following two shears:—

1_{AB}, giving extension along Y and contraction along Z,
2_{AB} „ „ „ Y „ „ X.

A circumferential tension (*t'* case B) gives rise to the following two shears:—

1_B, giving extension along X and contraction along Y,
2_B „ „ „ X „ „ Z.

It is necessary then to consider what effect these shear stresses, induced by quenching, have on the behaviour of a bar subjected to a tension (T) or a pressure (P) in the direction of the Z axis.

A pull, T, in direction of the Z axis gives rise to the following two shears:—

1_T producing extension along Z and contraction along X,
2_T „ „ „ Z „ „ „ Y.

A push, P, along the Z axis produces the following two shears:—

1_P giving contraction along Z and extension along X,
2_P „ „ „ Z „ „ „ Y.

It will thus be seen that the shear stresses induced in a bar of metal by sudden cooling have the effect of weakening certain layers of the bar as regards resistance to tension, and certain layers as regards resistance to compression. For the shear stress 1_T is applied along the same series of parallel planes as the stress 2_A, and although the stress 2_T is directly opposed by the stress 1_{AB}, the "yielding" of the material must be determined by its strength in the weakest direction. Similarly the stresses 1_P and 2_P are in the same directions as the stresses 2_B, 1_{AB}, so that the loss of elasticity exhibited by quenched material both as regards tension and compression has been accounted for. It may, however, be desirable to consider a little in detail what ought to be the behaviour under tension of, say, a bar of iron which has been subjected to the system of stresses described above. At the commencement of the loading the stress due to the applied load will be uniformly distributed over the whole section, but as soon as a very small load is applied, a long cylindrical layer of material (A'), which has been left by the sudden cooling under a stress of type 2_A very nearly equal to the "yielding" stress of the material, will yield. This yielding would continue to the enormous extent characteristic of a yield-point were all the material in the condition A'; but the weak layer, being surrounded by stronger material, the yielding is only allowed to continue to a very slight extent. This small yielding will, however, cause a redistribution of the internal stresses set up by quenching to

take place, and perhaps also a redistribution of the stress due to the applied load. This alteration in the distribution of the internal stresses must be such as to cause the surrounding strong layers to stretch elastically as far as the weak material has been permanently stretched. The alteration in the internal stresses will remain after the applied load is removed, as the material which has been permanently deformed will be unable to relieve the stronger material. The apparent permanent set which is shown with quenched material after the removal of applied load, may thus be due to the real permanent extension only of the weak layers, and to the elastic extension of the strong layers produced by the new distribution of internal stresses. This explanation, however, does not suffice, at least in the case of iron and steel, to explain the behaviour of a quenched rod under applied stress, for Diagrams 2 and 3 show that such a rod may be stretched further than is compatible with elastic extension—even supposing some of the iron to have been overstrained to the maximum in the most favourable direction, without stretching nearly far enough for the yield-point of the iron to have been passed. Hence in the case of iron and steel recourse must be had to the explanations which simply attribute the observed effects to the formation of allotropic modifications of the metal or to the changes caused by the transition of the carbon—always present—from one condition to another.

In conclusion, it may be recorded that pieces of the iron and steel specimens used in this research were polished, etched, and examined under the microscope. In the case of the steel specimens the change from the ferrite and pearlite structure shown with the annealed material to the martensite structure shown with the quenched steel was very striking. But in the case of the Lowmoor iron no difference was detected by the microscope in the structures of the annealed and of the quenched specimens, although, as shown by Diagram 3, the elastic properties in the two conditions were vastly different.

"Harmonic Tidal Constants for certain Australian and Chinese Ports." By THOMAS WRIGHT, of the Nautical Almanac Office. Communicated by Professor G. H. DARWIN, F.R.S. Received August 1, 1902.

Ballina (*New South Wales*), *Princess Royal Harbour* (*King George's Sound*), *Newcastle* (*New South Wales*), *Brisbane* (*Queensland*), *and Sydney* (*New South Wales*).

The tidal observations made at these five ports have been reduced by the aid of certain sums placed at my service by the Government Grant Committee of the Royal Society, and I am indebted to Professor

G. H. Darwin for the loan of the apparatus he devised to facilitate the summation of hourly tidal heights, and to the Hydrographer, Admiral Sir W. J. L. Wharton, who supplied me with the observations. The whole of the observations were reduced by the methods devised by Professor G. H. Darwin.*

The observations made at the three first-mentioned ports were derived from copies of continuous diagrams made by automatic tide gauges; those at Brisbane and Sydney were times and heights of high and low water. The observations in every case extended over a period of about 1 year, and were almost complete. The breaks in the continuity of the observations were so short that approximate values could be easily inserted by interpolation with very small risk of error.

From the automatic records at Ballina, Princess Royal Harbour, and Newcastle the hourly heights were read off to the nearest one-tenth of a foot. The range of the tide at these ports is small, and an attempt was made to use a smaller unit, one-twentieth of a foot. An experiment with one month's observations showed, however, that the hourly and daily sums for the month differed very slightly, whether the readings were taken to the nearest one-twentieth foot or to the nearest one-tenth foot only. Besides, when the diagrams for two consecutive days were placed end to end there frequently appeared to be a difference of at least one-twentieth of a foot between the end of one day's curve and the beginning of the next day's. For these reasons it was considered to be sufficient to work to the nearest one-tenth of a foot, and that length was adopted as the unit.

The heights being read off, the method followed was exactly that described by Professor Darwin, except in one detail. The S sheet (that is, the sheet which is used for obtaining the hourly sums for the S tides) was not used. These sums were made on the sheets on which the hourly heights were entered from the diagrams. As in Professor Darwin's method, the hourly sums were made in groups of days which could be built up into the 30-days' period for the S tides and into the 74-days' period for the other tides. The *daily* sums were made throughout the year (they are required for the long-period tides). By forming totals of these *daily* and *hourly* sums in appropriate groups they act as a check on each other, and the two sets of sums are settled.

The hourly heights for the first 74 days were then entered on the strips, the strips were pinned to the M sheet, and the additions made. The total of the 48 sums was checked against a corresponding total made up from the *daily* sums and the *hourly* sums for S. Agreement among these three totals is a check upon the copying on to the strips, and also upon the sums for M. This slight modification of forming the sums for *S* from the original heights as read off from the diagrams saves one shifting of the strips, and, if all goes well and the totals

* 'Roy. Soc. Proc.,' vol. 48, pp. 277—340, and vol. 52, pp. 345—389.

agree, it seems hardly necessary to check the entering on the strips. In all other respects Professor Darwin's methods were followed exactly.

As already stated, the observations made at Brisbane and Sydney were the times and heights of high and low water. They were reduced by Professor Darwin's method. The observations were split up into four groups, each covering about one-fourth of a year. Each of these groups was separately reduced, and means of the four values of κ and H for each tide was taken as the final constant. These separate values form a check on the work independently of the systematic method of verification which was adopted in each stage of the computation. In the case of the more important tides, the agreement among the four values is very close. In the case of some of the smaller tides the differences are somewhat greater, but not great enough to make any serious difference in predictions based upon the constants.

The constants for the five ports are given, with others, below. The small value of H (0·159 foot) for M_2 at Princess Royal Harbour gave rise to the suspicion that there was some error in the work. This value is, however, borne out by the value of H for M_2 at Batavia (Java) given in the American Tide Table for 1900. Batavia is there quoted as a "Standard Port for Reference" for King George's Sound, and the height there given is 0·154 foot, or only 0·005 foot different from that obtained for Princess Royal Harbour. Careful examination of the work showed, too, that the value given below is correct.

Hong Kong, Swatow, Whampoa, Cooktown, and Cairn's Harbour.

The constants for these ports have been deduced at various times during the past few years. Except in the case of some of the Hong Kong tides, constants for these ports have not yet been published, and the present opportunity is taken to include them with the others.

The observations made at Hong Kong, Swatow, and Whampoa were from records by automatic gauges; those at Cooktown and Cairn's Harbour were observations of times and heights of high and low water. They were reduced by the same methods as the observations at the other five ports. The observations for the Chinese ports were kindly supplied by the Chinese Customs authorities; those for Cooktown and Cairn's Harbour by the Hydrographer. There seemed reason to suppose that the observations at Whampoa had not been very good, and the results for the tides S_6, R, M_1, M_6, L, μ, 2SM, J, Mf, and M_m seem to be so uncertain that I have thought it best to omit them from the Table of Values. For a like reason the L tide is omitted from the results for Cooktown and Cairn's Harbour.

	Princess Royal Harbour, King George's Sound. 1876–7.	Newcastle, N.S.W. 1900.	Ballina. 1898.	Hong Kong. 1889.	Swatow. 1897–8.	Whampoa. 1894–5.	Brisbane Bar. 1865–6.	Sydney. 1888.	Cooktown. 1890.	Cairn's Harbour. 1892–3.
Latitude	35° 8′ S.	32° 57′ S.	28° 52′ S.	22° 18′ N.	23° 23′ N.	23° 5′ N.	27° 31′ S.	33° 52′ S.	15° 27′ S.	16° 55′ S.
Longitude	118° 0′ E.	151° 44′ E.	153° 33′ E.	114° 10′ E.	116° 39′ E.	113° 26′ E.	153° 0′ E.	151° 12′ E.	145° 15′ E.	145° 47′ E.
A_0	1·926	3·035	2·001	5·879	5·686	5·722	7·120	2·449	16·624	4·476
S_1 H =	0·019	0·018	0·015	0·018	0·065	0·119	—	—	—	—
S_1 κ =	186	25	86	139	106	118	—	—	—	—
S_2 H =	0·262	0·391	0·276	0·567	0·316	0·666	0·579	0·375	0·788	1·120
S_2 κ =	342	265	275	290	86	64	315	268	258	245
S_4 H =	0·012	0·006	0·003	0·007	0·025	0·003	—	—	—	—
S_4 κ =	204	289	246	37	216	16	—	—	—	—
S_6 H =	0·003	0·000	0·001	0·001	0·001	*	—	—	—	—
S_6 κ =	299	31	130	222	56	*	—	—	—	—
M_1 H =	0·023	0·022	0·004	0·060	0·019	*	—	—	—	—
M_1 κ =	340	76	220	100	48	*	—	—	—	—
M_2 H =	0·159	1·598	1·083	1·447	1·347	2·184	2·201	1·636	1·873	1·958
M_2 κ =	339	249	262	267	23	32	290	254	282	282
M_3 H =	0·011	0·013	0·004	0·051	0·038	0·100	—	—	—	—
M_3 κ =	6	346	195	328	341	190	—	—	—	—
M_4 H =	0·005	0·027	0·058	0·072	0·228	0·160	—	—	—	—
M_4 κ =	16	233	121	324	154	313	—	—	—	—

M_6	H =	0·002	0·018	0·025	0·017	0·053	*	—	—	—	—
	κ =	227	74	133	168	172	*	—	—	—	—
O	H =	0·417	0·289	0·310	0·949	0·765	0·815	0·325	0·337	0·299	0·407
	κ =	312	88	128	245	254	310	139	86	113	166
K_1	H =	0·623	0·513	0·454	1·191	0·941	1·068	0·592	0·419	0·287	0·872
	κ =	330	120	155	294	292	354	176	129	171	190
K_2	H =	0·074	0·127	0·072	0·134	0·119	0·288	0·158	0·102	0·215	0·305
	κ =	338	254	273	272	73	64	315	268	258	245
P	H =	0·172	0·153	0·139	0·388	0·269	0·351	0·196	0·139	0·096	0·290
	κ =	332	116	149	292	285	15	176	129	171	190
J	H =	0·037	0·035	0·031	0·032	0·025	*	—	—	—	—
	κ =	356	141	184	307	299	*	—	—	—	—
Q	H =	0·091	0·072	0·077	0·172	0·139	0·116	—	—	—	—
	κ =	291	165	103	228	242	326	—	—	—	—
L	H =	0·039	0·069	0·047	0·026	0·080	*	0·180	0·065	*	*
	κ =	349	238	286	284	34	*	258	237	*	*
N	H =	0·067	0·351	0·202	0·301	0·237	0·380	0·458	0·324	0·447	0·658
	κ =	17	235	254	255	358	16	288	250	239	269
ν	H =	0·020	0·123	0·018	0·012	0·074	0·047	—	—	—	—
	κ =	33	245	169	133	327	352	—	—	—	—
μ	H =	0·019	0·061	0·026	0·072	0·070	*	—	—	—	—
	κ =	16	226	335	237	176	*	—	—	—	—
R	H =	0·043	0·011	− 0·006	0·007	0·043	*	—	—	—	—
	κ =	192	213	347	60	180	*	—	—	—	—

	Princess Royal Harbour, King George's Sound. 1876-7.	Newcastle, N.S.W. 1900.	Ballina. 1898.	Hong Kong. 1889.	Swatow. 1897–8.	Whampoa. 1894–5.	Brisbane Bar. 1865–6.	Sydney. 1888.	Cooktown. 1890.	Cairn's Harbour. 1892–3.
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Longitude	118° 0′ E.	151° 44′ E.	153° 33′ E.	114° 10′ E.	116° 39′ E.	113° 26′ E.	153° 0′ E.	151° 12′ E.	145° 15′ E.	145° 47′ E.
T H =	0·038	0·024	0·016	0·035	0·015	0·036	—	—	—	—
T κ =	255	291	270	281	82	23	—	—	—	—
MS H =	0·015	0·062	0·043	0·067	0·103	0·144	—	—	—	—
MS κ =	268	252	199	301	200	359	—	—	—	—
2SM H =	0·027	0·026	0·037	0·026	0·025	*	—	—	—	—
2SM κ =	71	236	213	235	91	*	—	—	—	—
Mm H =	0·065	0·082	0·102	0·078	0·050	*	—	—	—	—
Mm κ =	135	198	2	101	298	*	—	—	—	—
Mf H =	0·064	0·051	0·097	0·083	0·069	*	—	—	—	—
Mf κ =	175	105	314	310	120	*	—	—	—	—
Msf H =	0·024	0·030	0·230	0·112	0·074	0·270	—	—	—	—
Msf κ =	240	330	45	40	0	59	—	—	—	—
Sa H =	0·328	0·232	0·413	0·466	0·467	0·484	0·109	0·093	0·346	0·202
Sa κ =	111	70	7	243	270	171	8	16	320	9
Ssa H =	0·235	0·074	0·063	0·280	0·258	0·135	0·005	0·008	0·051	0·050
Ssa κ =	97	201	257	97	96	92	156	97	36	157

NOTE.—A — indicates that the values of these tides were not determined. An * indicates that there is reason to believe that the determination of the tides is so imperfect that it is better to neglect them.

"On some Definite Integrals and a New Method of reducing a Function of Spherical Co-ordinates to a Series of Spherical Harmonics." By ARTHUR SCHUSTER, F.R.S. Received May 30,—Read June 5, 1902.

(Abstract.)

The expansion of a function $f(\theta)$ of an angle θ varying between 0 and π in terms of a series proceeding by the sines of the multiples of θ depends on the fundamental theorem,

$$\int_0^{\pi} \sin p\theta \sin q\theta \, d\theta = 0,$$

where p and q are integer numbers not equal to each other. Similarly if P_n denotes the zonal harmonic of degree n, $\mu = \cos\theta$, and

$$Q_n^{\sigma} = \sin^{\sigma}\theta \frac{d^{\sigma}P_n}{d\mu^{\sigma}},$$

the expansion of a function of θ in terms of a series of the functions Q_n^{σ} depends on the corresponding theorem,

$$\int_{-1}^{+1} Q_n^{\sigma} Q_i^{\sigma} d\mu = 0,$$

where i and n are two integer numbers not equal to each other. In many practical applications a continuous function is given by means of its numerical values at certain points, *e.g.*, for equidistant values of θ.

Such cases present no difficulty when Fourier's analysis is to be employed, because there is in that case a summation theorem exactly corresponding to the above integration theorem. If θ be replaced by $\rho\pi/n$, where ρ takes successively the values 1, 2, 3 . . . , the equation

$$\sum_{\rho=0}^{\rho=n-1} \sin(\rho p\pi/n) \sin(\rho q\pi/n) = 0$$

will hold true. This allows us to determine the coefficients in the case of problems in which discontinuous values of the function at equidistant points are known (*e.g.*, hourly readings of temperature or barometric pressure). If we assume that all Fourier coefficients beyond the nth vanish, n equations are obtained, each of which only contains one of the unknown quantities.

If it is desired to expand a function in terms of cosines, a slight

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modification must be introduced, the summation theorem in that case being

$$\tfrac{1}{2} + \tfrac{1}{2}\cos p\pi \cos q\pi + \sum_{\rho=1}^{\rho=n-1} \cos(\rho p\pi/n)\cos(\rho q\pi/n) = 0,$$

the first and second terms representing half the value of the product for $\rho = 0$ and $\rho = n$ respectively.

There is no corresponding summation theorem in the case of the functions Q_n^σ, and the application of the method of least squares leads to a series of normal equations, each of which contains *all* the other coefficients. This has been one of the great practical difficulties in obtaining an expression for the series of spherical harmonics for the earth's magnetic potential.

F. E. Neumann has tried to overcome the difficulty by calculating coefficients $a_1, a_2 \ldots a_q$ in such a way that

$$\sum_{\rho=1}^{\rho=q} a_\rho Q_n^\sigma(\mu_\rho) Q_i^\sigma(\mu_\rho) = 0.$$

Here $\mu_1, \mu_2 \ldots \mu_q$ are the quantities for which the values of the function to be represented are known. Neumann's process is equivalent to attaching weights proportional to a_ρ to the different observations, a proceeding against which theoretical objections might be urged.

2. The expansion in terms of a series of cosines and sines being so much easier than the direct expansion in terms of a series of the functions Q_n^σ, I have endeavoured to obtain the latter series by means of the former.

It is well known that a function of an angle θ, which is confined to the values lying between 0 and π, may be put either into the form

$$a_0 + a_1\cos\theta + a_2\cos 2\theta + \ldots a_p\cos p\theta + \ldots,$$

or into the form

$$b_1\sin\theta + b_2\sin 2\theta + \ldots b_p\sin p\theta + \ldots$$

The reduction to the series of spherical harmonics is accomplished by calculating and tabulating the coefficients in the series

$$\cos p\theta = A_\sigma^\sigma Q_\sigma^\sigma + A_{\sigma+1}^\sigma Q_{\sigma+1}^\sigma + \ldots A_n^\sigma Q_n^\sigma + \ldots,$$

$$\sin p\theta = B_\sigma^\sigma Q_\sigma^\sigma + B_{\sigma+1}^\sigma Q_{\sigma+1}^\sigma + \ldots B_n^\sigma Q_n^\sigma + \ldots$$

The choice between the cosine and sine series is open to us, but it appears that great simplicity is gained by taking the former series when σ is odd and the latter when σ is even. For in that case the coefficients A_n^σ and B_n^σ will all vanish, as long as n is smaller than p.

When it is therefore desired to retain only terms as far as the nth degree, the Fourier coefficients need only be calculated as far as $p = n+1$. The position of the earth's magnetic axis, *e.g.*, only depending on the terms of the first degree, is completely determined by the coefficients b_2 for $\sigma = 0$ and a_0, a_2 for $\sigma = 1$.

3. The symbolical representation of the results of this paper is much facilitated by the introduction of a separate symbol for the product of alternate factors, $n \,.\, n-2 \,.\, n-4 \,.\, .\, .\, 1$, if n be odd, or $n \,.\, n-2 \,.\, .\, .\, 2$ if n be odd. I propose to write $n\,!\,!$ for such products, and if a name be required for the product to call it the "alternate factorial" or the "double factorial." Full advantage of the new symbol is only gained by extending its meaning to negative values of n. Its complete definition may then be included in the equations

$$n\,!\,! = n(n-2)\,!\,!, \qquad 1\,!\,! = 1, \qquad 2\,!\,! = 2.$$

From this we may derive when n is negative and odd

$$n\,!\,! = (-1)^{\frac{n+1}{2}} \frac{1}{(-n-2)\,!\,!},$$

while for n negative and even, the factorial becomes infinitely large.

4. The calculation of the factors A_n^σ and B_n^σ depends on the values of the definite integrals

$$\int_{-1}^{+1} Q_n^\sigma \cos p\theta d\mu, \qquad \int_{-1}^{+1} Q_n^\sigma \sin p\theta d\mu,$$

and these may be made to depend on the values of the integrals

$$\int_{-1}^{+1} Q_n^\sigma \sin \lambda\theta d\mu \qquad \text{and} \qquad \int_{-1}^{+1} \mu Q_n^\sigma \sin \lambda\theta d\mu.$$

It is proved that

$$\int_{-1}^{+1} Q_n^\sigma \sin \frac{\lambda}{\theta} d\mu = c \frac{(n+\sigma-1)\,!\,!\,(\sigma+\lambda)\,!\,!\,(n-\lambda-2)\,!\,!}{(n-\sigma)\,!\,!\,(\sigma-\lambda-2)\,!\,!\,(n+\lambda+1)\,!\,!}, \text{ if } n-\sigma \text{ be even,}$$

$$= 0 \qquad \text{if } n-\sigma \text{ be odd,}$$

$$\int_{-1}^{+1} \mu Q_n^\sigma \sin \lambda\theta d\mu = c \frac{(n+\sigma)\,!\,!\,(\sigma+\lambda)\,!\,!\,(n-\lambda-3)\,!\,!}{(n-\sigma-1)\,!\,!\,(\sigma-\lambda-2)\,!\,!\,(n+\lambda+2)\,!\,!}, \text{ if } n-\sigma \text{ be odd,}$$

$$= 0 \qquad \text{if } n-\sigma \text{ be even.}$$

The factor c is equal to 2 or to π according as $\sigma+\lambda$ is even or odd.

5. The integrals

$$\int_{-1}^{+1} Q_n^\sigma Q_i^\rho d\mu$$

are obtained in the form of a series having a finite number of terms.

6. To find

$$\int_{-1}^{+1} Q_n^\sigma \sin p\theta d\mu \quad \text{and} \quad \int_{-1}^{+1} Q_n^\sigma \cos p\theta d\mu,$$

we may either express Q_n^σ or the trigonometrical functions in terms of a series of powers of $\sin\theta$. The second alternative leads to results which in general are more convenient.

If we put

$$C_0 = 1; \quad C_1 = p; \quad C_2 = p.\frac{p}{2}; \quad C_3 = p.\frac{p-1.p+1}{1.2.3};$$

$$C_\lambda = \frac{p}{\lambda!}\frac{(p+\lambda-2)!!}{(p-\lambda)!!};$$

$$B_0 = 1; \quad B_1 = p; \quad B_2 = \frac{p-1.p+1}{1.2}; \quad B_3 = \frac{p-2.p.p+2}{1.2.3}$$

$$B_\lambda = \frac{1}{\lambda!}\frac{(p+\lambda-1)!!}{(p-\lambda-1)!!},$$

we find if σ be even, p odd, and n even,

$$\int Q_n \sin p\theta d\mu = \pi\frac{(n+\sigma-1)!!(n-3)!!}{(n-\sigma)!!(n+2)!!}\left\{C_1.\sigma-1.\sigma+1\right.$$

$$-C_3\frac{\sigma-3.\sigma-1.\sigma+1.\sigma+3}{n-3.n+4}+C_5\frac{\sigma-5.\sigma-3.\sigma-1.\sigma+1.\sigma+3.\sigma+5}{n-3.n-5.n+4.n+6}$$

$$\left.-\ldots.\right\},$$

and if σ be even, p even, and n even,

$$=\pi\frac{(n+\sigma)!!(n-4)!!}{(n-\sigma-1)!!(n+3)!!}\left\{B_1.\sigma-1.\sigma+1-B_3\frac{\sigma-3.\sigma-1.\sigma+1.\sigma-3}{n-4.n+5}\right.$$

$$\left.+B_5\frac{\sigma-5.\sigma-3.\sigma-1.\sigma+1.\sigma+3.\sigma+5}{n-4.n-6.n+5.n+7}-\ldots.\right\}$$

If $n+\sigma+p$ be even, the integral is zero.

Similar equations are obtained for

$$\int_{-1}^{+1} Q_n^\sigma \cos p\theta d\mu.$$

7. The final results are expressed as follows :—

If g_n^σ denote the coefficient of $R_n^\sigma \cos\sigma\phi$ in the series of spherical harmonics when $R^\sigma = t_n^\sigma Q_n^\sigma$, and t_n^σ is a numerical coefficient, ϕ being

the longitude, the results of the investigation may be put into the form

$$g_n^\sigma = \sum_{p=0}^{p=n+1} a_p^\sigma r_n^\sigma, \text{ when } n \text{ is even, } \sigma \text{ odd, and } p \text{ odd.}$$

$$= \sum_{p=1}^{p=n+1} a_n^\sigma n_n^\sigma \quad ,, \quad n \;,, \text{ odd, } \sigma \;,, \quad ,, \quad p \text{ even.}$$

$$= \sum_{p=1}^{p=n+1} b_p^\sigma m_n^\sigma \quad ,, \quad n \;,, \text{ even, } \sigma \text{ even, } ,, \quad p \text{ odd.}$$

$$= \sum_{p=2}^{p=n+1} b_p^\sigma s_n^\sigma \quad ,, \quad n \;,, \text{ odd, } \sigma \;,, \quad ,, \quad p \text{ even.}$$

In these equations the factors $a_p^\sigma b_p^\sigma$ are the coefficients of the Fourier series (see § 2), and the quantities r_n^σ, n_n^σ, m_n^σ, s_n^σ are numerical quantities, which (as well as their logarithms) are given in tables at the end of the paper as far as $n = 12$, $\sigma = 12$, $p = 12$. By means of these tables the numerical work is reduced to a minimum, and the coefficients of the series may be obtained as far as terms of the 12th degree.

8. The proposed method is specially adapted to deal with problems like that of terrestrial magnetism, in which the function to be obtained as a series of spherical harmonics is not given directly, but by means of its differential coefficients. The force directed to the geographical north may by Fourier's analysis be obtained as a sum, the terms of which have the form $\cos \sigma\phi \cos p\theta$, and $\sin \sigma\phi \cos p\theta$ when σ is even, and the form $\cos \sigma\phi \sin p\theta$, $\sin \sigma\phi \sin p\theta$ when σ is odd. Integrating with respect to θ, the magnetic potential is obtained in a form such that the transformation into the series of spherical harmonics may be proceeded with. A separate expression of the magnetic potential is derived from the force directed to the geographical east.

"Contributions to a Theory of the Capillary Electrometer. II.—On an Improved Form of Instrument." By GEORGE J. BURCH, M.A. Oxon., F.R.S., Lecturer in Physics, University College, Reading. Received September 26,—Read November 20, 1902.

During the sixteen years that I have worked with the capillary electrometer I have had occasion to make between 150 and 200 instruments, and have therefore given naturally a good deal of thought to the problem of its construction. I have used eleven different forms, three of which are figured in my little book on the 'Capillary Electrometer in Theory and Practice,' reprinted from 'The Electrician,' 1896. A fourth is in use in the Physiological Laboratory, Oxford, for the research on nerve in which Professor Gotch and I have been engaged, and is in fact the improved form referred to on page 9 of my book.

It may be of interest to indicate briefly the points that must be observed in the design of the instrument.

It must be simple, easy to adjust and to clean, and with reasonable care not liable to be broken. Above all it must be suitable for use with objectives of short focus and wide-angle condensers. For this reason I adopted in my first projection-electrometers the plan of placing the capillary within a piece of thick-walled burette tubing of 1 mm. bore, half ground away so as to form a trough of semi-circular section. A piece of thin cover-glass serves as a front to this trough, the lower end of which dips into the dilute sulphuric acid, the liquid rising in it by capillary attraction to a sufficient height above the level of the U-tube to enable the microscope to be focussed on the capillary within.

In the first instrument of this type the trough was ground to fit the mouth of the U-tube like a stopper, and rested loosely in it. It was found, however, that as the acid loses or absorbs water with the changes of weather, the variations of level in the U-tube affect the adjustment of the trough, bringing the capillary sometimes too far from the cover-glass for good definition and sometimes dangerously near it.

Hence in the later improved type I fused the upper end of the glass trough on to a glass rod which was fixed firmly with adjusting screws to the brass support that held the capillary, the U-tube containing the acid being independently supported so that the end of the trough dipped into it. The short limb of the U-tube was made wide in order that the trough should not be likely to touch against it in the event of any accidental pressure on the instrument, and also because experi-

ence has shown that capillaries are less liable to become sticky with much acid than with very little. The only objection to this type is the difficulty of adjusting the capillary in so small a trough. The operation has to be effected under the microscope and is both delicate and tedious. When it has been completed the definition is perfect, and the instrument not unduly fragile. But in order to clean it the same process has to be gone through again.

I therefore determined to try an entirely different plan, on which the safety of the capillary should depend not on the rigidity of the supports by which the trough was fixed, but on their perfect flexibility and on the use of a trough so light that its entire weight might even be borne by the capillary.

Fig. 1 is a perspective diagram of the instrument in its final form, and figs. 2, *a*, *b*, *c*, and *d*, show the details of the trough which is the essential part. The support A is cut from a solid block of ebonite 9 cm. long, 5 cm. wide, and 2 cm. thick. It is first cut to shape, holes drilled for the binding screws E and F, and the piece B then separated from it by two saw-cuts.

V-shaped grooves are cut to receive the capillary C, which is firmly clamped under B by E and F. The longer limb of the U-tube D passes through a hole drilled lengthways through the lower end of A, which is slit about half way up with a wide saw-cut, so that it may be pinched together by the screw G. Adjustments for setting the capillary at right angles to the optic axis and parallel to the slit of the photographic recording apparatus, are provided for by the stout brass plate K, bent at right angles, one end of which is fastened by a binding-screw at L to the back of A, and the other by a similar screw M to the adjustible stand of the projection microscope. The brass plate K is so shaped that there is a space of about 3 mm. between it and the left-hand side of the ebonite support A, in order to leave room for the adjustment of the latter about L as a centre.

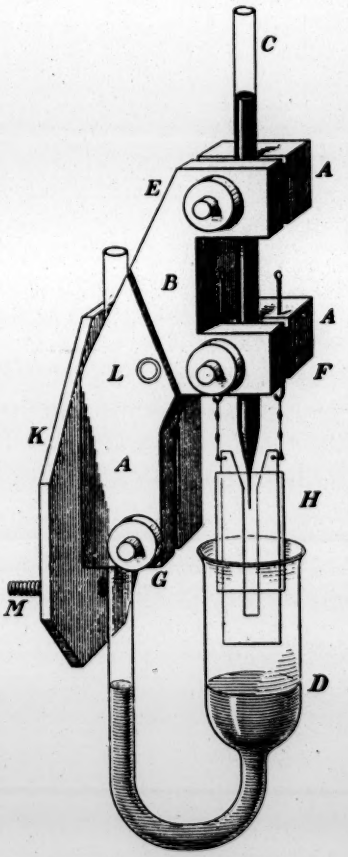


FIG. 1.—Half full size.

The construction of the trough is shown in the full-size diagrams, figs. 2, *a*, *b*, *c*. A piece of mica, such as is used for lamp-shades, is cut to the shape *a*, with a pair of scissors. Two or three thicknesses may

be taken if one is not enough. Four holes are drilled with a needle in the positions shown. A thin piece of the best clear mica is then laid on a pad of blotting paper, the piece *a* placed on it, and four corresponding holes pricked through with the needle, the piece being after-

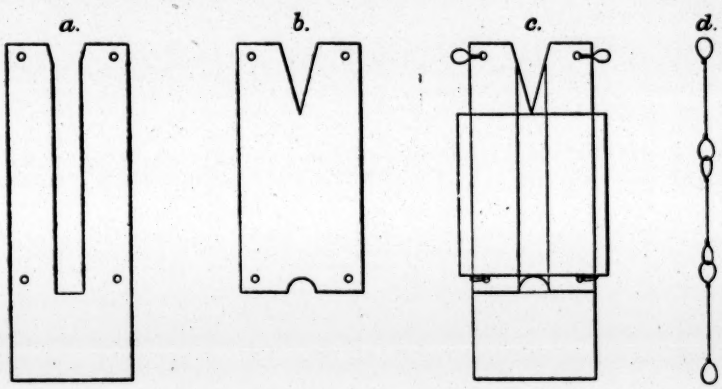


FIG. 2.—*a*, *b*, and *c*, full size; *d*, about twice full size.

wards cut to the shape *b*. Finally, *a* and *b* are fastened together by four little loops of No. 30 platinum wire, and the whole trimmed to shape with the scissors. The trough is then hung by two platinum chains (made of No. 30 wire with long links as shown enlarged at *d* (fig. 2), so that the acid may not creep up them) from the hooks shown in fig. 1. These hooks are best made of half-round wire, doubled like a linch-pin, sliding easily but firmly in holes on each side of the capillary, as in fig. 1.

The operation of putting in a capillary is as follows: The instrument is fixed to any convenient support by the screw M. The milled head G is loosened, and the U-tube D drawn down and turned aside. The whole instrument is then tilted backwards to an angle of 45° from the vertical. In this position the trough H hangs clear of the capillary. If the capillary has been already filled and connected with the pressure tube, the nuts E and F must be unscrewed far enough for the tube to pass sideways into the clamps, but a new capillary may be easily and safely inserted from below after merely loosening E and F. It must then be filled to within 2 cm.—not nearer—of the top, with recently distilled mercury from a perfectly clean pipette, connected with the pressure apparatus,* and some mercury forced through.

The screw M is then slightly loosened, and the instrument raised cautiously to a nearly vertical position. The trough H is adjusted by sliding the hooks from which it hangs up or down, or bending them, until the capillary rests against the centre of it—the apparatus being

* Full details of the pressure apparatus, the cleaning of the tubes, and the method of drawing capillaries were given in my book.

tilted back during each alteration. When these adjustments have been made, the inside of the trough is wetted by touching it with a glass rod dipped in dilute sulphuric acid of 25 per cent., and the apparatus is tilted forwards until the wet trough swings against the capillary, and sticks to it. A piece of thin cover-glass—or of mica if very high powers are to be used—slighly wider than the trough, is picked up with a pair of fine forceps, wetted on one side with the acid, and placed carefully against the trough, to which it adheres, holding it firmly against the capillary, the lower edge of the glass resting against the platinum loops with which the trough is fastened together (see fig. 2, *c*).

The U-tube is then turned back into position, cautiously raised until the lower edge of the glass just dips into the acid, and clamped by the screw G. Finally the trough is gently shaken or pushed to and fro in the plane of the mica, until the acid rises in it to the required height and all bubbles are expelled.

The trough is held together so firmly by surface-tension that it seems at first sight a difficult matter to take off the cover-glass without breaking the capillary. It may, however, be done with the greatest ease as follows: The screw G is loosened, and the U-tube D drawn down and turned aside. A small beaker filled with water is held in its place and raised until the trough H is completely immersed, when the slightest movement causes the glass to fall off. The apparatus is then tilted back so that the trough swings clear of the capillary, which may be washed or even wiped, and the trough dried, replaced, and a new cover-glass put on, in less than three minutes.

The definition, with these electrometers, is perfect. The capillary touches the cover-glass throughout its length, so that any dry objective can be used. The microscope should not, however, be left focussed on the capillary, lest the acid should chance to get between the objective and the cover-glass—an accident which I have known to happen in very damp weather.

My only fear in designing this instrument was lest contact with the mica should contaminate the acid and so spoil the tubes. There does not, however, seem to be any such effect. I have had some mica troughs in use for nearly three years, and have never once been troubled with a sticky capillary, and even with induction shocks they will stand more than the old type on account of the larger quantity of acid in the U-tube. They are easier to make than my old "normal type" on account of the straight capillary, and so far as I can judge they seem likely to supplant both it and my previous projection electrometer.

"On the Correlation of the Mental and Physical Characters in Man. Part II." By ALICE LEE, D.Sc., MARIE A. LEWENZ, B.A., and KARL PEARSON, F.R.S. Received November 3,—Read November 20, 1902.

(1.) In a first paper on this subject* we gave a brief account of our material—Miss Beeton's copies of the Cambridge anthropometric measurements with degrees added at the University Registry, and the school measurements carried out by assistance from the Government Grant Committee. This material will take years to exhaust, but the present notice gives further conclusions to be drawn from Dr. Lee's and Miss Lewenz's later reductions from this great mass of raw statistics.

(2.) In the first place we may refer to certain matters which arise directly from the first paper. In the discussion which followed the reading of that paper it was suggested that we ought not to correlate intelligence with absolute measurements on the head, but with their ratio to the size of the body. The answer made on that occasion was based on data not then published, namely, that there is no sensible correlation between intelligence and the absolute size of the body. Hence the correlation between intelligence and any ratio of body lengths must also be small. To show this algebraically let x_1 and x_2 be any two measurements, and $R_{x_1x_2}$ the ratio x_1/x_2; let $r_{y_1y_2}$ denote the coefficient of correlation of any two characters y_1, y_2; let v_x be the coefficient of variation of the quantity x, *i.e.*, be 100 times its standard deviation divided by its mean.† Then we have the following formulæ‡ :—

$$v^2_{R_{x_1x_2}} = v^2_{x_1} + v^2_{x_2} - 2v_{x_1}v_{x_2}r_{x_1x_2} \quad \ldots\ldots\ldots\ldots \text{(i)},$$

$$r_{iR_{x_1x_2}} = \frac{v_{x_1}r_{ix_1} - v_{x_2}r_{ix_2}}{v_{R_{x_1x_2}}} \quad \ldots\ldots\ldots\ldots \text{(ii)},$$

where i denotes intelligence and x_1, x_2 any other characters.

Clearly when r_{ix_1} and r_{ix_2} are both small $r_{iR_{x_1x_2}}$ cannot be large. Let L be length of head, B be breadth of head, and S be stature. Then in the case of the Cambridge graduates

$v_L = 3{\cdot}1839$,	$r_{LS} = 0{\cdot}2816$,	$r_{iL} = 0{\cdot}0861$,
$v_B = 3{\cdot}2836$,	$r_{BS} = 0{\cdot}1529$,	$r_{iB} = 0{\cdot}0450$,
$v_S = 3{\cdot}6958$,	$r_{LB} = 0{\cdot}3448$,	$r_{iS} = -0{\cdot}0056$.

* "On the Correlation of Intellectual Ability with the Size and Shape of the Head," 'Roy. Soc. Proc.,' vol. 69 (1902), pp. 333—342.

† 'Phil. Trans.,' A, vol. 187, p. 276.

‡ *Ibid.*, p. 279. (ii) is deducible by simple algebra in the method often indicated in this series of papers.

The v's and the physical correlations are due to Dr. W. R. Macdonell,* r_{iL}, r_{iB} were given in our first paper,† and r_{iS} was deduced from the following fourfold table:—

(A.) *Intelligence.*

		Honours.	Pass.	Totals.
Stature.	Over 69″	244	228·5	472·5
	Under 69″	280	258·5	538·5
	Totals	524	487	1011

If r_{iS} were really sensible, it would mean that honours men were slightly shorter than pass men. The only safe conclusion we can draw, however, is that stature is not correlated with place in degree examinations.

From the above results we find

$$v_{R_{SL}} = 4{\cdot}1435, \qquad v_{R_{SB}} = 4{\cdot}5530.$$

Hence we have

$$r_{iR_{SL}} = 0{\cdot}0712, \qquad r_{iR_{SB}} = 0{\cdot}0370.$$

That is to say, the correlations of intelligence with the ratios of length and breadth of head to stature are slightly smaller than the correlations of intelligence with the absolute head-measurements. The result predicted from the smallness of r_{iS} in the discussion on the paper here receives its exact numerical confirmation.

(3.) Since our school measurements were started, MM. Vaschide, and Pelletier have published in the 'Comptes Rendus'‡ a statement that although unable to find any relation between intelligence and length or breadth of head, they consider a relationship to hold between intelligence and the auricular height of head. Their process was of the following kind. They asked the school teacher to select ten intelligent and ten non-intelligent children, and then measured the heads of these two sets, and found their means. This was done for groups of three ages in boys and two ages in girls. The probable errors of the difference of the means of ten observations are not considered, and by exactly the same process that they reason that the auricular height is greater for the more intelligent children they might have deduced from their statistics that intelligent girls of 11 years have lower heads

* 'Biometrika,' vol. 1, pp. 188-9.

† 'Roy. Soc. Proc.,' vol. 69, pp. 335–6.

‡ 'Comptes Rendus,' Paris, vol. 133, 1901, pp. 551—553.

than intelligent girls of 9 years, and non-intelligent boys of 11 years lower heads than the same class of 9 years! Frankly, we consider that the memoir is a good illustration of how little can be safely argued from meagre data and a defective statistical theory.

Taking from our school data the auricular height of 2005 boys, and from the growth table based on the same material, reducing them to the age 12 as standard, we find

(B.) *Auricular Height of Head and Intelligence.*

Height.	Intelligent.	Slow.	Totals.
Above 127 mm.	481·5	584·0	1065·5
Below 127 mm.	415·0	524·5	939·5
Totals	896·5	1108·5	2005

Whence the correlation = 0·0161.

There is thus less correlation between auricular height and intelligence than between either breadth or length and intelligence; indeed, it is less than the probable error, and no weight can be laid on it whatever. The discovery of MM. Vaschide and Pelletier that the auricular height of school children is related to their intelligence seems to us quite incorrect for English boys, and unproven owing to defect of material and method even for French children.

It has been suggested by a sweeping critic, who clings to the *high* correlation of intelligence and head size, that our school head-measurements are of no value. To this we can only reply that in all cases where the measurements have been in the least doubtful the spanner has been returned and the measurements re-made. Further, if the absence of correlation between intelligence and head-measurements be a proof that the head-measurements have been taken badly or the scale of intelligence wrongly applied, how does it happen that high correlation comes out for the head-measurements of brothers, for all three cases, breadth, length, and height, and that its value is quite in keeping with the correlation between the intelligence of brothers? The existence of careless measurement or appreciation would have reduced these correlations also to near zero, as well as those on the characters on the *same* individual. We are forced to conclude that while our data give surprisingly consistent and uniform results for collateral heredity when we deal with upwards of twenty characters,* about half mental

* Results for seven mental and three physical characters were given in 'Roy. Soc. Proc.,' vol. 69, p. 155. These numbers have been more than doubled since that paper was published.

and half physical, they give with an equal weight the definite result that there is no *marked* correlation between intelligence and the size or shape of head in children.

(4.) While it seems desirable later to investigate specially the Cambridge data from the standpoint of the subject studied, as well as degree taken, we complete at present the list of other physical correlations with intelligence on the simple basis of honour and pass degree groups.

The following are the tables:—

Intelligence and Strength of Pull.

(C.) First Grouping.

Pull.	Honours.	Pass.	Totals.
Above 84 lbs.....	251	256·5	507·5
Below 84 lbs.....	273	229·5	502·5
Totals	524	486	1010

(D.) Second Grouping.

Pull.	Honours, 1st class.	Honours, 2nd, 3rd classes, and Pass.	Totals.
Above 84 lbs.	75	432·5	507·5
Below 84 lbs.	78	424·5	502·5
Totals	153	857	1010

Intelligence and strength correlation is from the first grouping $-0{\cdot}0765$, and from the second $-0{\cdot}0199$. Thus it would appear that from either grouping the honours men have slightly less strength of pull than the pass men, but as even this small amount is decreased when we group the first class men only together, such inferiority as there is seems to lie in the second and third class honours men. Taking the average, we may say that there is a negative correlation of $-0{\cdot}0482$ between intelligence and strength of pull. The probable error of the result, about 0·035, shows that very little weight can be attached to it.

(E.) *Intelligence and Strength of Squeeze.*

Squeeze.	Honours.	Pass.	Totals.
Above 85 lbs.	236·5	227·5	464
Below 85 lbs.	282·5	255·5	538
Totals	519	483	1002

The correlation between intelligence and strength in this case $= -0{\cdot}0242$.

This result, although it is less than its probable error, is again negative.

(F.) *Intelligence and Sight.*

This is judged in the Cambridge Anthropometric Laboratory by the distance at which the test type can be read.

Sight.	Right eye.	Honour.	Pass.	Totals.
	Over 61″	259·5	239	498·5
	Under 61″	249·5	223	472·5
	Totals	509	462	971

Forty-one men on our cards were unclassed—10 in 1st class, 5 in second, 1 in third, and 25 poll-men. This was possibly due to defective sight, or even to the loss of the right eye, because the strength of the left eye was sometimes given; we have not ventured to group these unclassed cases, however, with the short-sighted division.

The correlation between intelligence and long sight $= -0{\cdot}0049$. This is far less than the probable error of the result, but is again negative.

(G.) *Intelligence and Weight.*

Weight	Honours.	Pass.	Totals.
Over 10 st. 13 lbs...	258·5	226	484·5
Under 10 st. 13 lbs.	265·5	261	526·5
Totals	524	487	1011

The correlation between intelligence and weight $= 0{\cdot}0459$, and is thus very slightly larger than its probable error.

Now, it has sometimes been argued that in any investigation of this kind, it is desirable to take not absolute weight, but its ratio to stature or some power of stature. Let W = weight, S = stature, and n = any power; let $R_n = W/S^n$, and v be a coefficient of variation, and r one of correlation, i standing for intelligence.

Then

$$v^2_{R_n} = v^2_W + n^2 v^2_S - 2n v_W v_S r_{SW} \quad \ldots\ldots\ldots\ldots\ldots \quad \text{(i)},$$

$$r_{iR_n} = \frac{v_W r_{iW} - n v_S r_{iS}}{v_{R_n}} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad \text{(ii)}.$$

But

$$v_S = 3{\cdot}6958, \qquad r_{SW} = 0{\cdot}4860,$$
$$v_W = 10{\cdot}8300, \qquad r_{iW} = 0{\cdot}0459,$$
$$r_{iS} = -0{\cdot}0058,$$

from results already given for the Cambridge data. Hence, calculating v_{R_n} from (i) for $n = 1$, 2, and 3, we deduce

r_{iR_1} = correlation of intelligence with ratio weight to stature = 0·0540,

r_{iR_2} = " " " (stature)^2 = 0·0555,

r_{iR_3} = " " " (stature)^3 = 0·0503.

There is no substantial difference between any of these correlations and that for intelligence and absolute weight. As they were found indirectly by formulæ, it seemed desirable to test at least one of them directly. Accordingly Miss M. Beeton found the ratios of weight per inch of stature for 1012 Cambridge men. The resulting table was as follows :—

(H.) *Intelligence and Weight per inch of Stature.*

	Honours.	Pass.	Totals.
Over 2·224 lbs. per in.	258·5	222	480·5
Under 2·224 lbs. per inch ..	265·5	266	531·5
Totals....	524	488	1012

The distribution is sensibly the same as that of the table for absolute weights, and the correlation comes out 0·0604, *i.e.*, it differs only by 0·0064, or about one-fifth of the probable error from the value of the correlation obtained indirectly.

We may then, I think, conclude that whether we take absolute weights or the ratio of weight to stature, honours men are slightly heavier than poll-men. Summing up the whole of our examination thus far of the Cambridge measurements we may say that :

The honours men, and presumably therefore the more intelligent class, are slightly heavier and have slightly longer and broader heads; they are not quite as tall nor as strong, whether strength be measured by pull or squeeze, and are slightly shorter-sighted than the poll-men, or presumably the less intelligent class. In no single case, however, is the correlation between intelligence and the physical characters sufficiently large to enable us to group the honours men as a differentiated physical class, or to predict with even a moderate degree of probability intellectual capacity from the physical characters of the individual.

(5.) While the above and the previously published results exhaust the Cambridge data, as long as we preserve the division into honours and poll-men, much more remains to be done on this material when we consider subject groupings among the Cambridge graduates, or when we turn to the much wider range of both physical and mental characters recorded in our school measurements.

A preliminary inquiry may, however, be recorded here as bearing upon a rather vexed question at the present day, namely, the relation of athletics to health and intelligence. In our school measurements we had three categories: Health—divided into the classes: *Very Strong,* Strong, Normally Healthy, Rather Delicate, Very Delicate.* Ability or Intelligence—was divided into six classes: *Quick Intelligent, Intelligent, Slow Intelligent, Slow, Slow Dull, Very Dull.*

Lastly, we had the alternative category—*Athletic, Non-athletic.* By *Athletic* we understand not only fondness for out-door exercises and games, but good performance in them. There was a control entry in the schedules under the heading *Games or Pastimes*, in which not only what the children *liked*, but in addition what they were *good at*, had to be entered. We were thus in a position to make that triple correlation between health, ability, and athletic power, which seems really needful, if a sane judgment is to be made on the part athletics should play in the school curriculum.

The following tables give the relations between health and ability, ability and athletic power, and health and athletic power:—

(I.) *Health and Intelligence.* 2253 *Boys.*

Health.	Quick intelligent, intelligent.	Slow intelligent, slow, slow dull, very dull.	Totals.
Very strong, strong..	415	453	868
Normally healthy ...	461	542	1003
Rather delicate, very delicate.....	128·5	253·5	382
Totals	1004·5	1248·5	2253

* *Strong* in these categories equals *robust.*

The correlation dividing at the *Strong* is 0·0820.

The mean of the other divisions (i) dividing at the *Delicate*, and (ii) putting the *Slow Intelligent* with the *Intelligent*, gave 0·0835. We conclude, therefore, that there is a sensible, but not marked correlation between good health and intelligence.

Taking, however, health and athletics we have the table:—

(J.) *Health and Athletics.* 1743 *Boys.*

	Very strong.	Strong.	Normally healthy.	Rather delicate.	Very delicate.	Totals.
Athletic ...	91	447·5	497·5	120	3	1159
Non-athletic	9·5	98·5	293·5	166·5	16	584
Totals.....	100·5	546	791	286·5	19	1743

The correlation between healthy and athletic dividing between *Strong* and *Normally healthy* is = 0·4570, a very marked relationship.

Next, taking intelligence and athletics, we find:—

Intelligence and Athletics. 1708 *Boys.*

	Quick intelligent.	Intelligent.	Slow intelligent.	Slow.	Slow dull.	Very dull.	Totals.
Athletic ...	159·5	421·75	355·5	158·75	40·5	12	1148
Non-athletic	46	163·25	187·5	99·75	48·5	15	560
Totals	205·5	585	543	258·5	89	27	1708

Dividing between intelligent and slow intelligent we find the correlation between intelligence and athletic character is 0·2133.

This result may be exhibited also in the percentages of athletic and non-athletic boys who fall under each class of intelligence:—

Percentages of Athletic and Non-athletic Boys under each grade of Intelligence.

	Quick intelligent.	Intelligent.	Slow intelligent.	Slow.	Slow dull.	Very dull.
Athletic ...	14	37	31	13	4	1
Non-athletic	8	29	32	18	9	3

The relationship between keenness for combined with capacity in games and general intelligence is here manifest.

Certain other correlations with the athletic character may be just noticed without giving the tables. The athletic boy is popular (0·3250) and noisy (0·3452), and this although popularity is not found to be directly correlated with noise. He is slightly self-conscious (0·0761), and is more likely to be fair than dark (0·0391). His temper tends to be quick rather than sullen (0·2207), as the following table, based on 1664 cases, will show :—

Percentages of Athletic to Non-athletic Boys for each Temper.

	Quick tempered.	Good-natured.	Sullen.
Athletic	21	68	11
Non-athletic.....	12	74	14

To sum up, then: While the intelligent are only *slightly* the more healthy, the athletic are *notably* the more healthy element in the community. Further, the athletic are considerably more intelligent than the non-athletic; they are the more popular and more noisy element; and they tend to quick rather than sullen temper. We may in general terms describe the athletic boy as healthy, quick-tempered, and intelligent when compared with the non-athletic boy. He certainly under all three headings should make a better soldier than the non-athletic, and it is hard to discover any statistical evidence in *school* life for such expressions as "the flannelled fool at the wicket," or "the muddy oaf at the goal." What happens in later life can only be determined when ample statistics are available for reduction and comparison. Failing such data, we can argue only from the vaguest of impressions.

"Note upon Descending Intrinsic Spinal Tracts in the Mammalian Cord." By C. S. SHERRINGTON, M.A., M.D., F.R.S., and E. E. LASLETT, M.D. Vict. Received November 5,—Read November 27, 1902.

In the course of experiments upon the paths of nervous conduction in the spinal cord of the mammal, one of us observed* very numerous and wide departures from the fourth so-called "law" of Pflüger. That "law" states that "Reflex-irradiation in dem Rückenmarke nach Oben, resp. Vorn, gerichtet ist; also gegen die Medulla oblongata."† The observation of the above-mentioned exceptions rendered desirable a search for more detailed evidence of intrinsic spinal paths running in the aboral direction. We therefore set about inquiring into the existence of spinal paths connecting the activity of segments situate nearer the head with segments lying further from the head. Such evidence is obtainable with some experimental difficulty, but it has been eventually forthcoming, and amounts to demonstration of the microscopic course of the channels involved. It is some main features of these latter that we desire to record in the present communication.

METHOD.

The method employed has been that of the Wallerian nerve-fibre degeneration, but with a novel feature in the mode of application of the method. For the purpose in view the ordinary establishment of a cross-lesion in the spinal cord is futile. The secondary degeneration then produced befalls, in the spinal region under investigation, all nerve-fibres having their perikarya headward of the cross-lesion, whether those perikarya lie in the cerebral hemisphere, basal ganglia, mid-brain, cerebellum, bulb, or cord itself. It is obviously then impossible to identify which particular ones, if any, of the degenerate nerve-fibres are coming from the cord-segments whose nerve-tracts are the special object of inquiry. To obviate this difficulty, we have adopted a method which may be termed a method of "*successive degeneration.*" The method consists in producing two or more successive degenerations with allowance of a considerable interval of time between them. In the piece of cord to be examined, a first degeneration is allowed time enough to remove all the tracts descending from sources other than those the immediate object of inquiry. This is a procedure which requires in our material, at shortest, 9 or 10 months to complete. When the time is complete, the cord is left, as it were,

* "Croonian Lecture," 'Phil. Trans.,' B, 1897.

† 'Die sensorischen Functionen des Rückenmarks der Wirbelthiere, nebst einer neuen Lehre über die Leitungsgesetze der Reflexionen' (p. 73), Berlin, 1853.

like a cleaned slate, on which once more a new degeneration can be written without fear of confusion with a previous one. The cord is then ready for receiving the lesion which shall cause degeneration of the particular tracts whose existence is suspected. After a period suitable for the full development of the new degeneration, the cord is treated histologically by the Marchi method, and the microscopical examination proceeded to. This method resembles in principle a method employed with noteworthy results by Münzer.* This author performed on the new-born rabbit a first lesion (*e.g.*, removal of one cerebral hemisphere, injury of mid-brain or cord), and later, in the animal when grown, proceeded to establish a new lesion which was thus uncomplicated by the part already separated: thus "Gudden's *agenesic* atrophy" was made to precede the degeneration desired for study.

One of the experiments made by Münzer and Wiener (1895) deals with the problem undertaken by ourselves. After semi-section of the spinal cord of the new-born rabbit at the last dorsal segment, they performed total transection two segments further back when the animal was grown. Behind the second section they found "as many fibres degenerate on the semi-sected side as on the intact side." If not "decussation fibres" these fibres must evidently be of intra-spinal origin in the anterior lumbar region. As to their being decussation-fibres, Münzer says they are, on the contrary, from the grey matter of the same side as the semi-section, a statement which our own results in the dog in the same and other regions endorse.

In our experiments the cord of the dog has been used, and total transection has not been the final, but the first step in the procedure. This course was chosen in order to completely exclude all chance that fibres from sources not the object of inquiry could complicate the second lesion. In order to ensure complete transection, we have in almost all our experiments exsected and ablated a short segment of the cord, instead of simply severing it across. The exsection was made immediately in front of those spinal segments whose system of descending fibres in the cord was to be looked for. Then after an interval, which we found by experience must not fall short of 260 days, the second lesion, usually some form of partial section, was performed, and a further period of about 20 days was allowed for degeneration. The procedure of total transection prior to the lower limiting lesion has an additional advantage in the lesser interference with the local circulation of the cord in the final lesion. Better in these respects this plan offers, however, considerably greater difficulties than its converse. We have, in spite of all care, lost a number of experiments in the long intervals necessary to elapse while

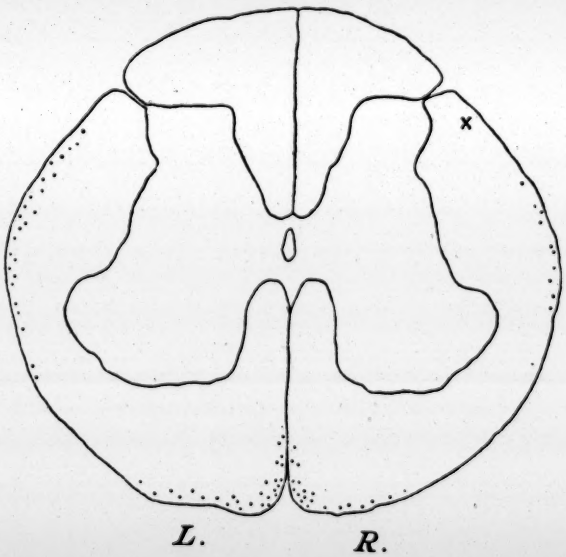
* E. Münzer, with Wiener, 'Prager Medicin. Wochenschrift,' 1895; also 'Monatschr. f. Psychiat. u. Neurol.,' vol. 12, p. 241, 1902.

the cord is ripening for the second operation. We have, however, obtained thirteen successful complete experiments: in these the shortest interval has been 260 days, the longest 568 days. Among these is included two in which, for special reasons, the partial cross-lesion of the cord was made precedent to the total transection.

Results.

The spinal segments examined as sources of aborally-running fibre-systems have been posterior cervical, anterior thoracic, mid thoracic, posterior thoracic, and anterior lumbar. From all these regions our experiments demonstrate that copious aborally-running fibre-systems spring. Thus, the accompanying fig. 1 shows, for instance, tracts of fibres in the 5th lumbar segment which have their origin in cells of the 2nd thoracic segment.

Fig. 1.



Cross-section of the cord of the dog at a level in the anterior part of the 5th lumbar segment; Marchi preparation. The section reveals the topography at that level of the aborally-running fibre-system of the 1st and 2nd thoracic segments. The 8th cervical segment of the cord had been completely exsected and ablated. A partial translesion (rather more than a semi-section) was made the left half of the 3rd thoracic 568 days subsequent to the removal of the 8th segment. The exact extent of this second lesion was determined subsequently by microscopic examination in serial preparations, and its limits will be described and figured in a fuller communication. The dots indicate, in a way mentioned in the text (p. 120), the density and extent of the degenerate tracts of fibres. L = left side; R = right side.

Speaking generally, of the fibres composing the aborally-running systems springing from the grey matter of the spinal segments

examined, we find there may be distinguished two sets. For physiological description it is in some ways convenient to regard the length of the spinal cord as divisible into regions; thus, a brachial for the fore limb, a thoracic for the trunk, a crural for the hind limb, a pelvic for pelvic organs, a caudal for the tail, and so on. A reflex initiated *viâ* an afferent path of one such spinal region may evoke its peripheral effect by efferent paths of a spinal region other than that to which the original entrant path belongs. Such a reflex has in a former paper by one of us* been termed a "long" spinal reflex, in contradistinction to reflexes whose centripetal and centrifugal paths both belong to one and the same spinal region. The latter reflex it was proposed to term "short."† Analogously, in the aborally-running fibre-systems of the spinal segments examined, by our experiments fibres of two categories are found, one a set passing beyond the limits of the spinal region in which they arise, the other not passing beyond those limits. The former we would term "long spinal," the latter "short spinal" fibres. In each of these main categories there can be distinguished fibres of various intermediate length.

Again, the fibres of each of the above two categories may be classified into two sets or tracts, according to their topography relatively to the cross-section of the cord. Fibres of both of the above categories are situate both in the lateral columns and in the ventral columns of the cord. It is useful, at least for descriptive purposes, to indicate this by terminology. We thus recognise in the aborally-running intrinsic spinal fibre systems the following sets or tracts: (α) *Ventral short fibres*, (β) *ventral long fibres*, (γ) *lateral short fibres*, (δ) *lateral long fibres*. It must be added that the distinction into lateral and ventral is somewhat artificial, as there exists often, especially in the case of the "short" fibres, no distinct gap between the ventral and lateral fields of distribution of the fibres in the transverse area of the cord.

In regard to the "long" fibres, we find that in all the regions examined by our experiments there is no evidence of decussation of these tracts. This statement does not exclude the possibility that the collaterals or the fine ultimate terminals of these fibres may in some cases penetrate in the grey matter across to synapses in the crossed side of the grey matter. We have at present no reliable microscopical evidence for or against such a possibility. But all our evidence is consentient that the fibres themselves do not pass from the white columns of one side of the cord into those of the crossed side, that is, do not in the ordinary sense decussate.

A similar statement seems also to hold true for the "short" fibres, thus confirming Münzer:‡ it is certainly true of the majority of the

* C. S. Sherrington, "Croonian Lecture," 'Phil. Trans.,' 1897.

† *Ibid.*

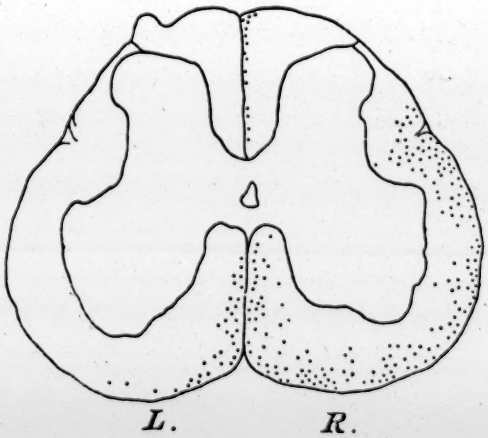
‡ 'Prager Medic. Wochenschr.' 1895.

short fibres; but analysis of our material makes us hesitate to positively affirm that it is true for all of them. A small proportion of the short fibres *may* decussate, at least in the sense that short fibres arising in perikarya belonging to one lateral half of the cord may find their way into the white ventrolateral columns of the crossed half. We do not affirm, however, that any of even our "short" fibres do decussate, we simply affirm our present inability to deny that a small proportion of them may do so.

Some of the "long" fibres are very long, both in the lateral and in the ventral columns of the cord. Thus, some of those arising from perikarya in the 6th and 7th cervical segments we have traced into the sacral region, *i.e.*, through nearly thirty spinal segments, both in the lateral and in the ventral columns. The rule pointed out by one of us in a previous paper,* that the long fibres in the spinal cord tend to lie nearest the surface of the cord, is well exemplified in these intrinsic spinal systems.

Besides fibres in the ventrolateral columns the aborally-running fibre-systems of spinal origin include fibres in the dorsal columns.

FIG. 2.

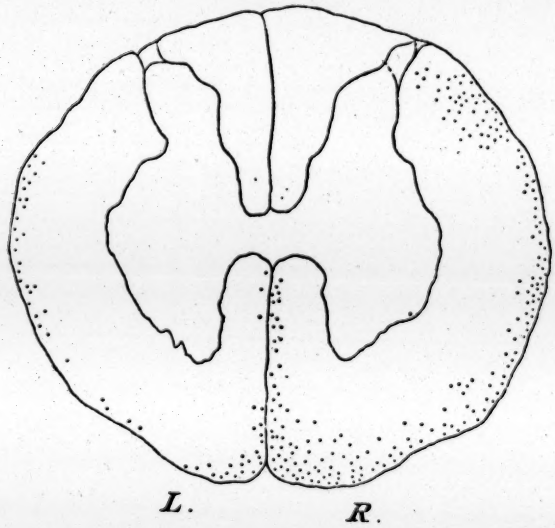


Cross-section of the spinal cord of the dog at the level of the 1st sacral segment; Marchi preparation. The section reveals the topography at that level of the aborally-running fibre-system of the nerve-cells of the 1st and 2nd lumbar segments. A short length of the 13th thoracic segment of the cord had been completely exsected and ablated. A partial translesion (rather more than a semi-section) was then made through the right side of the 2nd lumbar segment in its anterior levels 290 days subsequent to the total exsection. The extent of this second lesion was accurately determined later by microscopic examination in serial preparations; its exact limits will be described in a fuller communication. The dots indicate, in the way mentioned in the text, the density and extent of the tracts of degenerate fibres. L = left side; R = right side.

* 'Journ. of Physiology,' vol. 14, p. 298. Cambridge and London, 1893.

These are less numerous. We defer detailed description of them, together with further detailed description of the ventral and lateral tracts above-mentioned, until a fuller communication dealing with the whole subject. The general features of the topography can be gathered better from the two accompanying figures than from any even lengthy

FIG. 3.



Cross-section of the spinal cord of the dog at the level of the anterior part of the 4th lumbar segment: Marchi preparation. The section reveals in left half of the cord the topography at that level of the aborally-running fibre-systems arising in perikarya of the grey matter of the left half of the 6th and 7th cervical segments. Total transection of the cord had been performed through the 8th cervical segment 268 days subsequent to left semi-section at the 5th cervical segment. Thirteen days only was allowed for the development of the degeneration after the second lesion. The degeneration may not therefore appear so extensive as it might have done later, but its localisation is probably the more precise. The exact extent of the semi-section was found by subsequent microscopic examination in serial preparations to amount almost accurately to a full section of the left half of the cord; the detailed limits will be described in a fuller communication. The degeneration in the right half of the lumbar cord figured includes aborally-running fibres derived not merely from the spinal cervical grey matter, but from bulbar and cerebral cources as well; and these are practically inextricably commingled one with another. The dots signify, in the way mentioned in the text below, the density and extent of the degenerations. L = left side; R = right side.

textual description. All the figures have been drawn with the camera lucida upon squared proportional paper, and the squares on the paper have been made to correspond with squares in an engraved eye-piece. All the drawings are to exactly the same scale. The dots signify degenerate nerve-fibres, but the number of dots does not of course represent the absolute number of degenerate fibres, but falls far short

of it. Extreme care has been taken, however, to make the number of the dots bear fairly accurately a general proportion to the density of the degeneration, and the same proportion in one drawing as in another.

"The Inter-relationship of Variola and Vaccinia." By S. MONCKTON COPEMAN, M.A., M.D. Cantab., F.R.C.P. Communicated by LORD LISTER, F.R.S. Received November 13,—Read November 27, 1902.

The term "variolæ vaccinæ" employed by Jenner, as a synonym for cow-pox, has been generally accepted as affording evidence that in so naming this disease, "small-pox of the cow," he was desirous of placing on record his belief that cow-pox, or vaccinia, was intimately related to human small-pox, if indeed it were not directly derived from it.

This theory, however, appears to have found but scanty favour in Jenner's day, and even at the present time the value of the practice of vaccination is, by some, impugned on the plea that inoculation of one disease—cow-pox—could not be expected to exert any really protective influence against the ravages of small-pox—a disease considered by them of totally different origin.

In the hope of obtaining definite information on the subject, many observers, during the long period which has elapsed since the introduction of vaccination, have set themselves the task of attempting to solve, by experimental methods, the problem of the true relationship of vaccinia to variola.

These attempts have been, for the most part, directed to the possibility of giving rise to cow-pox by the introduction, in one or another manner, of the virus of small-pox into the system of the bovine animal. In the great majority of such attempts, which have been much more numerous than is generally supposed, the results have been entirely negative, although so numerous have been the experimenters, who from time to time have attacked the problem, that the total number of instances in which an apparently successful result has been obtained, is now considerable.

So far as I am aware, the first recorded experiments are those of Gassner of Gunsberg, who, in 1801, succeeded, after no less than ten fruitless attempts, in directly variolating a cow with small-pox virus. The lymph thus obtained was employed for the vaccination of four children, from whom other seventeen were subsequently vaccinated. None of these exhibited any signs of small-pox.

It is impossible, here, to do more than mention the names of other investigators who have engaged in research of this nature at various times from the commencement of the last century up to the present time. I therefore merely append in the foot-note* a list of such names, placed, as far as possible, in chronological order. It is a noteworthy fact that every observer mentioned, with the exception of Chauveau and his colleagues of the Lyons Commission, and Martin, claim to have obtained, on one or more occasions, positive results as regards the production of typical vaccinia, generally after one or more removes from the animal originally variolated.

But it must, I think, be admitted that many of the earlier experiments, more particularly, are practically worthless owing to the conditions under which they were carried out. Some of the main objections are based on the frequently concomitant use of vaccine and of variolous lymph on the same animal, and the want of care as to the cleanliness and freedom from vaccine contamination of lancets and "points" used in the experiments.

As regards my own work, carried out on similar lines to those adopted by previous observers, and of which a full account was published in the 'Journal of Pathology and Bacteriology,' in 1894, it may here be mentioned that I obtained an undoubtedly successful result in one series only, out of four attempts. In four subsequent variolation experiments, carried out several years later (1901) in connection with work, a detailed account of which is set out in the

* Chronological list of observers who have carried out variolation experiments on bovines :—

1801.	Viborg	Copenhagen
1828.	McMichael	Egypt.
1830.	Sonderland	Barmen.
„	Numann	Utrecht.
1832.	Macphail	Baltimore.
1836.	Thiele	Russia.
„	Martin	Attleborough, Mass.
„	MacPherson	India.
1839.	Reiter	Munich.
„	Ceely	England.
1840.	Badcock	„
1863-65.	Chauveau	Lyons.
1868.	Shortt	India.
1871.	Chauveau	Lyons.
1881.	Voigt	Hamburg.
1886-90.	Fischer	Carlsruhe.
1889.	King	Madras.
1890-91.	Eternod and Haccius	Geneva.
1892.	Simpson	India.
„	Hime	England.
„	Klein	„
„	Copeman	„

present paper, my attempts at direct transference of human small-pox material to the calf met with no success.

All my earlier experiments were conducted at the Brown Institution, in order to avoid any possibility of contamination with vaccinia. As a further precaution new scalpels were used, which were invariably first carefully sterilised in the flame of a spirit lamp, and, after use, the table was, on each occasion, thoroughly washed with carbolic acid (1 in 20), while during the intervals of use it was kept exposed to the air under an open shed. Similar precautionary measures have been observed throughout the course of my later work.

The difficulty experienced by myself and the numerous other investi gators, to whom reference has already been made, in attempts to transmit human small-pox directly to bovines, whether cows or calves, is not infrequently cited as a reason for regarding with distrust the theory expounded by Jenner, that cow-pox, whether carried through the horse as intermediary host or not, was originally derived from small-pox in the human being.

But a great deal, at any rate, of the small-pox which was prevalent at the time that Jenner lived and wrote was of that comparatively mild variety which, under the name of inoculated small-pox, was intentionally produced in healthy subjects, with the object of thereby conferring protection against subsequent attack by the disease in virulent form.

So mild indeed at times were the results of inoculations in the hands of such operators as Adams and the brothers Sutton that, as we learn from contemporary records, in many instances but little obvious effect was observed, with the exception of the local vesicle arising at the site of insertion of the small-pox virus, and the patients suffered but little inconvenience. Thus, more particularly in certain of Adams' cases, as may be gathered from his own account of the circumstances, the visible effect produced so closely resembled the results then beginning to be known as following on the Jennerian process of vaccination, that numbers of his patients were with difficulty persuaded that he had not, contrary to their desire, intentionally vaccinated rather than variolated them. The gradual evolution of a strain of lymph of such tenuity, according to Adams himself, was obtained by attention to the mode of life and general treatment of persons undergoing the process, together with careful selection of the sources (preferably the primary vesicle) from which the virus was obtained.

The majority of persons thus inoculated are not likely to have been incapacitated, as the result of the operation, to a much greater extent than are those who undergo efficient vaccination at the present day, and doubtless, therefore, they would be, for the most part, capable of following their ordinary avocations during the progress of the induced

disorder. On the other hand, this would hardly have been possible in the case of persons contracting small-pox in the ordinary way, among whom the disease was apt to exhibit such virulence as to account for the death of perhaps 50 per cent. of those attacked.

Not only were the effects following on inoculation comparatively mild, but the disease in this form was intentionally brought into many country districts which otherwise might not have become invaded by small-pox. In the light of these facts, it has for some time past been borne in upon my mind more and more convincingly that it was probably from the *inoculated* form of small-pox, rather than from the ordinary variety of the malady, that much, at any rate, of the cow-pox, in the pre-vaccination era, was derived. It is not difficult to understand how that the cracks so often found on the udders of cows might become infected by a milker with fingers contaminated by contact with the inoculation sore upon his arm.

I determined therefore, if possible, to put the matter to the test, and, learning that in Nubia, in Burmah, and in certain parts of India the inoculation of small-pox is still practised, I made numerous endeavours to obtain the necessary material, but unfortunately without success.

In default, therefore, of inoculated small-pox in the human subject, I made trial of the monkey, which, as I have shown in a previous communication to the Royal Society, is readily susceptible to the disease, the various phases of which in this animal closely resemble those observed in man, but in a much milder form; the occurrence of a generalised eruption being exceptional.

The different series of experiments, protocols of which I append, have been carried out at intervals, determined mainly by the possibility of procuring the necessary small-pox material. The work was commenced in April, 1898, with a supply of small-pox lymph received from the Medical Officer of Health for Middlesborough, in which town an epidemic of the disease was then in progress. For subsequent supplies I am indebted to the Medical Officer of Health and the Medical Superintendent of the Small-pox Hospital at Glasgow, to the Medical Superintendent of the West Ham Small-pox Hospital at Dagenham, near London, and to the Medical Superintendent of the Hospital Ships of the Metropolitan Asylums Board.

The methods employed in the investigation have been briefly as follows:—

Collection of Material for Inoculation.

In the first instance this was obtained in a manner similar to that formerly employed in obtaining human vaccine lymph. Discrete vesicles, mature, but still containing clear lymph, on one or another portion of the body of a patient suffering from small-pox, were punctured

with a sterilised lancet, and their fluid contents received into fine capillary tubes, which were subsequently sealed in the flame of a spirit lamp to admit of transport. This operation, however, is a most laborious one, and was subsequently abandoned, at my suggestion, in favour of collection, in the post-mortem room, of vesicle pulp at a suitable stage of the eruption, by means of a small Volkmann's spoon, after the fashion now invariably used in the Government lymph laboratories in obtaining, from the calf, material for the production of glycerinated lymph.

After removal from the body the small-pox pulp is first carefully weighed, and then ground up in a small glass mortar, with the gradual addition of usually four times its weight of a sterilised 50 per cent. solution of pure glycerine in normal saline solution. After thorough emulsification, what is not required for immediate use is stored in tubes, resembling small test-tubes, which are then corked, sealed with liquefied paraffin to which carbolic acid has been added, and set aside in a chamber kept at a temperature a few degrees above freezing point. Both storage-tubes and corks are sterilised before use.

Bacteriological examination by the method of plate-culture often shows a comparatively small number of extraneous micro-organisms in a specimen of small-pox emulsion prepared in the manner described, but whenever possible it has been stored at a temperature of about 15° C. for some weeks prior to using it for inoculation.

Species and Age of Monkeys Inoculated.

For my original experiments on the transference of human small-pox to the monkey, a brief account of which was presented to the Royal Society in 1893, I employed the rhœsus monkey, for the reason that Professor Sherrington and myself had, at the time, a stock of these animals, which had been obtained for other experimental work. Having at that time obtained successful results in every one of my inoculations, I employed the same species of monkey in the greater number of the experiments comprised in the present research. As, however, during the progress of the work I learnt that Dr. Eilerts de Haan, who, in Batavia, had been working on similar lines to myself, had made use most successfully of the macaque monkey, I also obtained a few specimens of this species, in order to compare the results of variolation in these animals with those that I had previously observed in the rhœsus monkey. But after two or three inoculations of the macaque with small-pox material, I came to the conclusion that the results following on the operation were not ordinarily as typical as in those experiments in which rhœsus monkeys had been employed. At the same time the macaque is in this country more expensive and more difficult to obtain than the rhœsus, so that I reverted to the use of the latter species in subsequent work.

It would appear also that, as in the human subject, young animals are more susceptible to small-pox than are adults, since it was in those instances in which monkeys probably not more than a year old were variolated that the most successful results were obtained. In one instance, however, in which the monkey was believed to be not more than a few months old, the extremely fine downy hair, after shaving, grew again so rapidly as to render somewhat difficult the photographing of the effect produced by the operation.

Mode of Operation and Collection.

In the earlier experiments inoculation of the monkey with human small-pox emulsion was carried out by rubbing it well into scarified patches or linear incisions of the skin of the upper arm or of the inside of the thigh, after previous shaving and cleansing of the skin. Subsequently, however, in accordance with the suggestion of Dr. de Haan, a shaved area on the back of the animal was utilised for inoculation. In this situation the results of the operation were found to be equally good, and there is less liability of damage to the vesicles from the monkey scratching itself.

The eruption having arrived at maturity, after the lapse of a period extending from five to eight days from inoculation, the altered epithelium was removed either with a small Volkmann's spoon or by scraping with a scalpel, after cleansing the inoculation area, between the blades of pressure forceps. The resulting epithelial pulp was then rubbed up in a small glass mortar, with the gradual addition of about six times its weight of normal saline solution, containing, when it was desired to preserve and purify the emulsion, 50 per cent. of glycerine.

Experience has shown that in monkeys a year or more old, which have been inoculated, the vesicular stage of the eruption is at its height, as was formerly observed in the human subject, by the eighth day; but in younger animals the process tends to be hastened, and in some of the later and most successful cases, the eruption was completely vesicular as early as the sixth day (120 hours). The particular *breed* of monkey does not appear to exert any influence in this respect.

Transference to the Calf and Human Subject.

The methods employed for transference of the localised disease in the monkey, after one or more passages through that animal, to the skin of the calf need not be set out in detail, being similar to those ordinarily used in the process of calf vaccination But it may here be stated that at no stage of the investigation have these experimental calves been brought into contact with, or even placed in the same room as, the calves used in the current work of the Government Vaccine Establish-

ment. They were fed and otherwise attended to by a man specially detailed for the purpose. All instruments employed for the vaccination of monkeys, calves, or children were previously sterilised by boiling or passing through the flame of a spirit lamp. Attention may perhaps be called to the fact that the skin of the scrotum in the calf affords a specially favourable site for inoculation experiments, especially if, when the incisions are made, the skin is made tense by pressing down the testicles. The first transference from the monkey to the calf does not usually afford a perfect result. Indeed a second, third, or even later passage from calf to calf was usually required before the most typical vesiculation was obtained.

In certain cases children were vaccinated with lymph obtained from the experimental calves, and in all instances the resulting vaccination ran a perfectly normal course. With lymph of similar origin I also successfully vaccinated myself. But none of the strains of vaccine lymph, derived originally from human small-pox in the manner described, have been brought into general use.

PROTOCOLS OF EXPERIMENTS.

First Series.

February 21, 1898.—Glycerinated samples of small-pox lymph received this day from Medical Officer of Health of Middlesbrough. Patients living and aged respectively 20, 27, and 34 years; all had been vaccinated in infancy.

April 1.—At Brown Institution, inoculated small rhœsus monkey with small-pox emulsion of 21.ii.98 in five linear incisions on left arm, and in fourteen on abdomen, after previous shaving and cleansing of the skin, by means of soap and water, followed by warm boric acid lotion. Monkey isolated in separate room and attendant vaccinated as a precautionary measure.

April 5.—All insertions on both arm and abdomen evidently "taking."

April 8.—Distinct vesiculation at site of all incisions on arm and most of those on abdomen. Monkey ætherised and substance of vesicles removed with sharp spoon into small previously weighed and sterilised test-tube. Scrapings weighed (0·6 gramme) and ground up with six times the weight of 50 per cent. watery solution of glycerine. Resulting emulsion taken up into twelve capillary glass tubes.

April 13.—Monkey looks well. All incisions healed up. No sign of generalised eruption.

Calf Experiments.

April 9.—At the Animal Vaccine Establishment, Mr. Stott inoculated Calf No. 1 (No. 4363) on two scarified patches, in twelve incisions on scrotum, and forty-four in perineum and on abdomen, with contents of two capillary tubes of glycerinated pulp prepared from vesicles of monkey. Incisions made with scalpel previously sterilised.

April 12 (72 hours).—Practically nothing to be seen.

April 14 (120 hours).—All insertions on scrotum appear to have "taken," and, in addition, four (not quite so well) on abdomen. Insertions on perineum seem to have failed. Large bullous-looking vesicle on upper scarified patch.

From this and from the vesicular lines on scrotum and abdomen collected pulp by scraping, after clamping with compression forceps.

The same day (April 14), Calf No. 2 (No. 4369 in A.V.E. records) was inoculated on perineum, scrotum, and abdomen with material obtained from Calf No. 1.

April 19.—All inoculated lines, with exception of two on abdomen, "taken" well, eruption being markedly vesicular. Vesicles clamped and scraped; pulp being immediately employed for inoculation of Calf No. 3 (No. 4673 in A.V.E. records) in a number of long incisions on the perineum, scrotum, and abdomen.

April 24.—All lines of incision "taken" well; eruption perfectly typical of vaccinia.

From this calf, six children vaccinated at A.V.E. same day.

May 1.—Children returned for inspection, in ordinary course. All vaccinations completely successful. Photographed arms of two of these children, which presented most perfect eruption.

About a month later I hunted up the parents of all six children, when I learnt from the mothers' statements that in every case the vaccination had pursued a perfectly normal course.

Second Series.

March 3, 1900.—At the West Ham Borough Hospital, Dagenham, I removed small-pox vesicles from body of a man, æt. 56, who had died 24 hours previously from semi-confluent form of disease. Material removed in test-tube and placed in ice-chest.

March 5.—After removal of some shreds of epithelium for histological purposes, the remainder (0·25 gramme) ground up with twice its weight of 50 per cent. solution of glycerine, and the greater portion stored in amber-coloured capillary tubes. The test-tube was afterwards swabbed out, and cover-glass specimens made for microscopical examination.

With some of the glycerinated emulsion inoculated, at the Brown Institution, rhœsus monkey (young female) on shaved area of back, about 3 inches by 2 inches, previously well washed with warm boric acid solution. Incisions twelve in number made "en échelon."

March 8.—Inoculation has evidently "taken," as tips of each incision are distinctly raised, and whole prospect is that of a typical calf vaccination of about same age (72 hours).

March 12.—Eruption beautifully perfect; edges of vesicular portion a little irregular, and centre of each line of incision occupied by commencing "crust." No general eruption visible. Two photographs taken.

Removed lower half of vesicular area with sharp spoon (upper portion left in order to watch further development), and glycerinated resulting pulp. Material used for inoculating Monkey No. 2 and also a calf (No. 606).

March 12.—*Monkey No.* 2 inoculated in fourteen incisions with glycerinated pulp from Monkey No. 1. Technique as before.

March 19.—Eruption not so perfect as in Monkey No. 1. Vesicles not so defined and plump. Monkey very wild, and has made sites of incisions bleed by dashing from side to side of cage, which may be in some degree the cause. Photograph taken. Vesicular pulp removed and ground up with four times its weight of dilute glycerine. With some of this emulsion *Monkey No.* 3 (young male rhœsus) inoculated same day in ten linear incisions on shaved area of back.

March 26.—All insertions "taken" well. Photographed. Material collected and glycerinated, pulp being diluted about fifteen times, by mistake. Some used

for inoculation in twelve insertions of *Monkey No.* 4 (young male rhœsus) same day.

April 2.—All places have "taken" well, although material used for inoculation had been so diluted. Photographed. Vesicles scraped and pulp glycerinated.

Calf Experiments.

March 12.—Inoculated Calf No. 1 in half dozen long linear incisions by method usually employed in current vaccinations at Government Establishment, with glycerinated pulp from Monkey No. 1.

March 16.—Lines of incision slightly raised and red.

March 17.—Dr. Fremlin found few small vesicles had developed. These he clamped, inserting material obtained thereby on Calf No. 2.

March 21.—There were evident signs of "taking" at all po nts of insertion, the lines of incision being elevated and with a tendency to vesiculation. But appearances not considered sufficiently typical to permit of material removed being utilised for vaccination of children.

At this point, owing to unforeseen circumstances, this particular series of experiments was discontinued.

THIRD SERIES.

February 25, 1901.—At the Jenner Institute inoculated medium-sized macaque monkey with small-pox emulsion, made by working up scrapings from P.M. cases of the disease (received from Dr. Thomson, of the Belvedere Hospital, Glasgow), in a small amount of pure glycerine. Technique as in previous experiments. Incisions made on the monkey's back with blunt scalpel, which had lost its temper by constant passing through the flame, so that all incisions did not apparently penetrate to the true skin.

March 4.—"Taken" well, though not throughout all insertions as in first monkey of the last series, but as failure had only occurred where there was no mark of incision, it was probably for reason mentioned above, as the eruption which had appeared was good. Not markedly vesicular; lines of incision which were covered with slight crust being surrounded by a pinkish papular eruption. Photograph taken.

Scraped with aid of compression forceps, and rubbed up material in small amount of NaCl 0·7 per cent. solution.

Monkey No. 2 inoculated this day, with emulsion of scrapings from Monkey No. 1. Technique as before.

March 16.—All insertions "taken"; slightly more vesicular than in No. 1. Photographed. Compression forceps applied and scrapings removed and rubbed up in small mortar with NaCl solution.

Monkey No. 3 inoculated immediately, on the back, with emulsion of material obtained from No. 2. Animal very young; hair downy and not easily shaved.

March 18.—All insertions have "taken" and have wide whitish vesicular margin, but appearance rather spoiled as hair on back has grown so rapidly. Lymph oozed up when compression forceps applied. Photographed. Scrapings rubbed up with NaCl solution, of which small quantity was used immediately for inoculation of Monkey No. 4. Remainder glycerinated, tubed and stored in ice-chest for future trial on calf.

March 23.—Monkey No. 4, an old animal, had not apparently taken as well as No. 3, so series was discontinued.

April 20.—Monkey No. 3 vaccinated in six incisions on outside of thigh with current vaccine lymph of known potency.

April 27.—No result, although monkeys not previously protected take vaccinia as successfully as in the human subject.*

Calf Experiments.

March 22.—Calf No. 1 (1332), at the Government Animal Vaccine Establishment, inoculated with small quantity of glycerinated emulsion of vesicular pulp from Monkey No. 3.

March 27.—Tiny papules and vesicles which had made their appearance along lines of incisions removed with Volkmann's spoon and glycerinated.

March 29.—Material collected on March 27 inserted into three long incisions on Calf No. 2 (1342).

April 3.—Fifth day. Lines of all three incisions occupied by good vesicles, Photograph taken by Dr. Green. Vesicle pulp (0·37 gramme) removed and glycerinated.

April 24.—Calf No. 3 (1390), inoculated (with portion of material collected on April 3) in thirty-six incisions on abdomen and scrotum.

April 29.—All insertions had "taken" well, vesicles surrounded with slight pink areola. Vesicles scraped and pulp glycerinated. Emulsion stored in ice-chest.

October 3.—Four c.c. of this emulsion used for vaccination of calf at the Jenner Institute, by numerous linear incisions extending nearly whole length of abdomen, after manner usually employed at Government stations.

October 8.—Appearance indistinguishable from normal vaccination. 63 grammes of vesicle pulp collected and glycerinated.

The glycerinated emulsion prepared from material removed from the calf on October 8 was subsequently employed for the vaccination of other calves, a strain of lymph being thus obtained which continued to give excellent results both on children and calves. But the strain was never brought into general use, and all the glycerinated emulsion remaining was eventually destroyed.

Fourth Series.

April 29, 1901.—At the Jenner Institute young rhœsus monkey shaved on back, as in previous experiments, and inoculated in a dozen linear incisions with glycerinated emulsion of S.P. vesicle pulp received from Dr. Thomson, of the Belvedere Hospital, Glasgow, on March 26, 1901.

April 2 (120 *hours*).—Had "taken" so well that I decided to collect; lines of incision distinctly vesicular. After taking photo, washed inoculated area, and removed pulp with aid of clamp forceps. Thin lines of altered epithelium came off as in a good calf vaccination. No "crusting."

Monkey No. 2 inoculated same day with material obtained as above and subsequently triturated in small glass mortar with small quantity of equal parts of glycerine and normal saline solution.

May 9 (120 *hours*).—Every insertion "taken" successfully. More markedly vesicular in places than Monkey No. 1. Photograph taken, followed by usual process of collection and glycerination af vesicle pulp.

Monkey No. 3 inoculated on back in eight diagonal incisions. Emulsion remaining over taken up into capillary tubes, of which two given to Dr. Fremlin for trial on calf at A.V.E. on May 15.

May 13 (120 *hours*).—Eruption of perfectly vesicular character along course of all incisions made, the centre in each instance being occupied by thin linear crust.

* Copeman, 'Journal of Pathology and Bacteriology,' May, 1894.

Drs. Blaxall and Fremlin, on seeing the animal, both described eruption as being equal to that which in case of calf vaccination would be entered in official records as v.g. (very good). Monkey photographed and vesicle pulp collected and glycerinated. Portion of emulsion used same day for inoculation of Monkey No. 4. Another portion handed over to Dr. Fremlin for trial on calf at A.V.E.

May 18.—Monkey No. 4 "taken" well. Vesicle pulp collected, emulsified, tubed and stored in ice-chest.

Series not continued beyond this stage, as laboratory man had failed in attempts to obtain further supply of young rhœsus monkeys.

Calf Experiments.

May 8.—Emulsion of vesicle pulp from *Monkey No.* 1, used at A.V.E. for inoculation of Calf No. 1418.

May 13.—Dr. Blaxall noted "No vesiculation, slight thickening in one line."

May 15.—Material from Monkeys No. 2 and No. 3, inoculated on calves at A.V.E. by Dr. Fremlin.

May 20.—Both calves had "taken" to a certain extent, the result being most marked in calf inoculated from Monkey No. 3. Material collected, glycerinated, and stored. Portion subsequently handed to Medical Director of Jenner Institute for further trial.

May 26.—Inoculated calf at A.V.E. with emulsion of second removal from Monkey No. 3.

May 31.—Perfect vesicular eruption along course of all incisions. General effect indistinguishable from that obtained with the current vaccine lymph of the Government Establishment.

In view of successful results following on vaccination of children with former lymph stocks raised in similar fashion, it appeared unnecessary to employ this particular lymph for infantile vaccinations. But on vaccinating my own arm with it direct from the calf, I succeeded in raising by the eighth day a fairly typical vesicle, an effect in excess of that obtained by me on my own person at previous attempts at vaccination.

No further transference of this lymph was attempted.

My first series of experiments had not long been concluded, when I came across a reference to an account of similar work which had been carried out by Dr. Eilerts de Haan. The reference occurred in a paper by Dr. Bruno Galli-Valerio,* and on hunting up Dr. de Haan's original paper, which is entitled, "Vaccine et Rétrovaccine à Batavia,"† I found that it contained an account of a lengthy series of experiments on the variolation of monkeys and on the transference of the resulting affection to calves.

Dr. de Haan's work proved of special interest to me for the reason that, quite independently, we had been able to corroborate one anothers' work, except as regards the transference of the strain of variola vaccine to the human subject—a final test which Dr. Eilerts de Haan did not, as he says, feel justified in attempting, in view of the unfortunate experience of Chauveau, in connection with his abortive

* 'Centralblatt für Bakteriologie,' March 28, 1899, p. 380 *et seq.*

† 'Annales de l'Institut Pasteur,' 1896, p. 169.

attempts at variolation of the cow. Dr. de Haan's own words may be quoted:—"Je reconnais qu'il manque à ma démonstration d'avoir rapporté la variole mitigée du singe sur l'homme; c'est une expérience que je ne me suis pas cru en droit de faire. L'expérience de Chauveau enseigne à être prudent, et je ne me croirais autorisé à faire cette tentative que si le vaccin ordinaire dont je me sers me manquait au moment d'une épidémie. Mais j'espère qu'on répétera mes expériences à ce sujet."

For the purpose of his experiments Dr. de Haan made use of the macaque monkey (*Macacus cynomolgus*), which is common in the Dutch East Indies, and therefore was readily obtainable. His inoculations were, in each instance, made on a portion of the animal's back, which was first shaved and then cleansed with soap and water, followed by a solution of boric and salicylic acids. In his first series of variolations of the monkey, the small-pox lymph employed was obtained from a Javanese child, no statement however being made by him as to the age of the child, whether or not it had ever been vaccinated, or at what stage of the disease the lymph was taken. Transferred to the monkey, this lymph gave rise, in a week's time, to well-marked vesicles at the site of inoculation, while, in addition, a few papules were observed on the lips and the extremities. Seven subsequent primary variolations were, however, successfully carried out on monkeys, in only one of which was any evidence of generalization observed.

From the contents of the vesicles of the monkey first-mentioned, a second was inoculated, which in seven days developed vesicles at the inoculated points only. From this second monkey, in due course, a third animal was inoculated; from this a fourth, and so on, through a series of seven monkeys.

From the sixth monkey of this series, a calf was inoculated, which five days later presented an appearance indistinguishable from a typical vaccination. From the seventh monkey also, of the series, a calf was inoculated, with the result again, that after an interval of five days, perfect vaccine vesicles appeared at the site of each insertion of the lymph. From this calf another was vaccinated with complete success.

In a second series of experiments, lymph at the fourth remove, in the monkey, from human small-pox, gave rise to perfect vesicles when inserted on the skin of a calf, and the strain of vaccine lymph thus obtained was carried on successfully, through eight removes from calf to calf. Monkeys and calves all failed to react to subsequent inoculation with the strain of vaccine lymph in current use.

The results of my own experiments may be briefly summarised as follows:—

In each of the separate series of experiments the human small-pox lymph or pulp was first inoculated directly on calves, and in every instance, so far as could be observed, with altogether negative results. But with monkeys success was as invariably obtained, and when, after one or more passages through this animal, the contents of the local inoculation vesicles were employed for insertion on the calf, an effect was now produced which, after two or three removes in that animal, was indistinguishable from typical vaccinia.

Moreover, from the contents of vesicles raised in this manner on the calf, a number of children have, in turn, been vaccinated, some of whom were afterwards kept under observation for as long a period as a couple of months.

Every such vaccination "took" normally, and in no case was any bad result subsequently observed by myself, or reported by the parents of the children; no "generalisation" of the eruption occurring in any instance.

In conclusion, I desire to call attention to the somewhat remarkable fact that a mild and strictly localised form of small-pox, such as is induced in the monkey by the inoculation of material from cases of the generalised disease in man, should, when transferred to the calf, "take" readily with the production of a vesicular eruption of non-infectious character in that animal, whereas it is well known that successful transference of small-pox direct from man to calf can only be accomplished with the utmost difficulty.

The experimental results obtained in the course of the research, an account of which has been set out in this paper, all tend, then, to confirm the view that the vaccinia of Jenner's time was derived, in all probability, from a comparatively mild form of human small-pox.

In addition, I think it will be admitted that the work has afforded conclusive evidence of the essential identity of the virus of small-pox and cow-pox or vaccinia.

"On the Similarity of the Short-period Pressure Variation over Large Areas." By Sir NORMAN LOCKYER, K.C.B., F.R.S., and WILLIAM J. S. LOCKYER, M.A., Ph.D., F.R.A.S. Received October 18,—Read December 4, 1902.

[PLATES 1 and 2.]

In a paper presented in June last to the Society,* we pointed out the existence of a short-period oscillation of barometric pressure over the Indian area corresponding generally with a variation in the percentage number of prominences recorded on the sun's limb. This oscillation was further shown not to be limited to the Indian area, but to be marked at a far distant station, as Cordoba, in South America.

The present paper, which is a continuation of this investigation, was undertaken to extend the research over a larger area.

The monthly means of the pressure variations for each station have been divided as previously into two periods, namely, those months in which the pressures are above and those in which they are below the normal, the normal being the mean pressure for the whole period under investigation in each locality.

In dealing with large areas, it happens that during the same period of time (that is generally but not invariably six months), the pressure is above the normal in some places, and below the normal in others; the similarity of the curves representing the variation of the mean for this period, from year to year, indicates therefore that, in one case, a rise in the curve denotes that the pressure is higher, and, in the other, that the pressure is not so low as usual.

The accompanying curve (Plate 1) illustrates the variations of pressure which have been analysed. Commencing with Indian pressures (as represented by Bombay) the area was gradually extended to Ceylon (Colombo), Java (Batavia), Mauritius, and finally to Australia (Perth, Adelaide and Sydney).

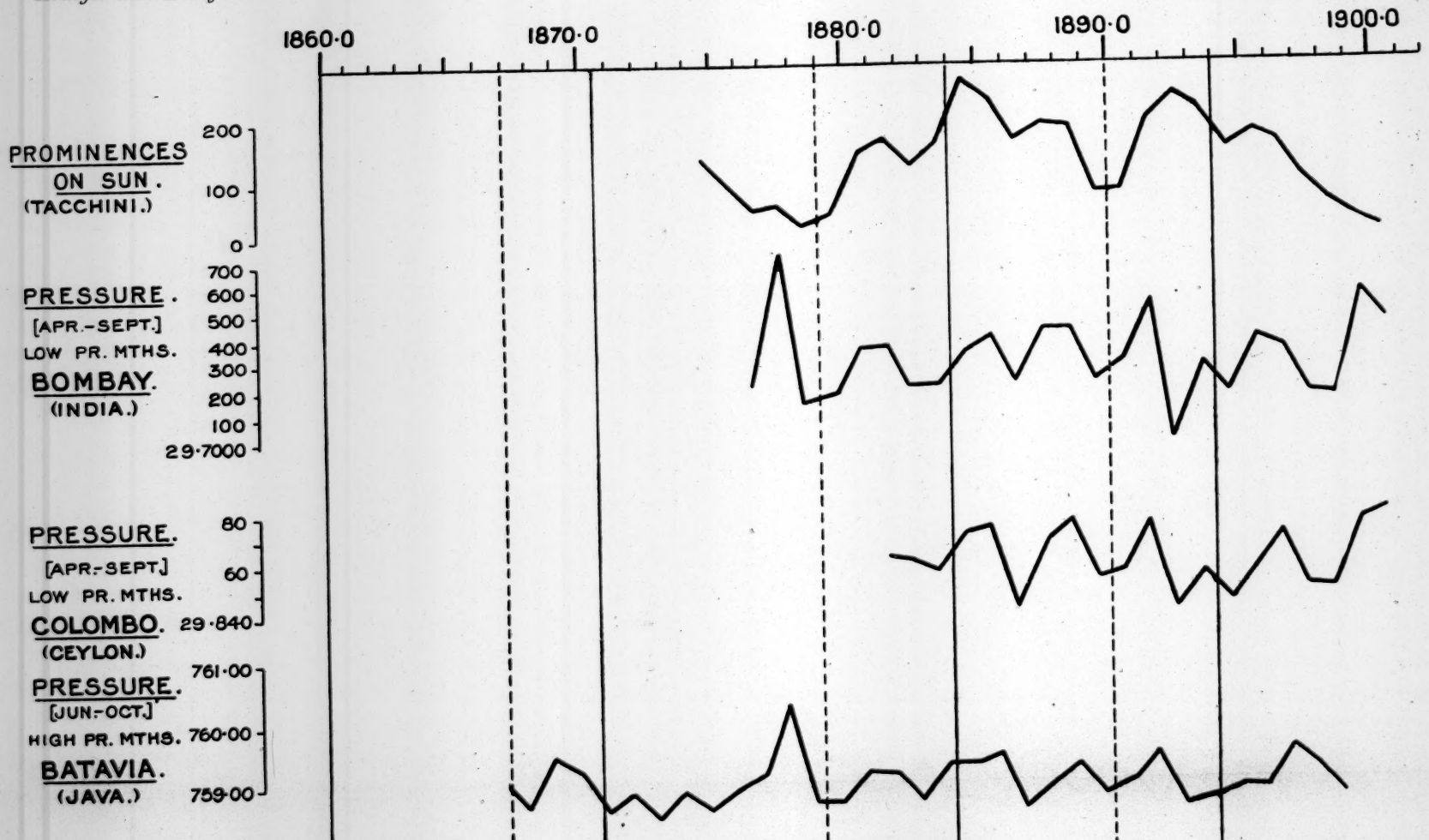
In this set of curves about the same months are in question, so that the pressure variations refer in the northern hemisphere to the low pressure (summer) months, and in the southern hemisphere to the high pressure (winter) months.

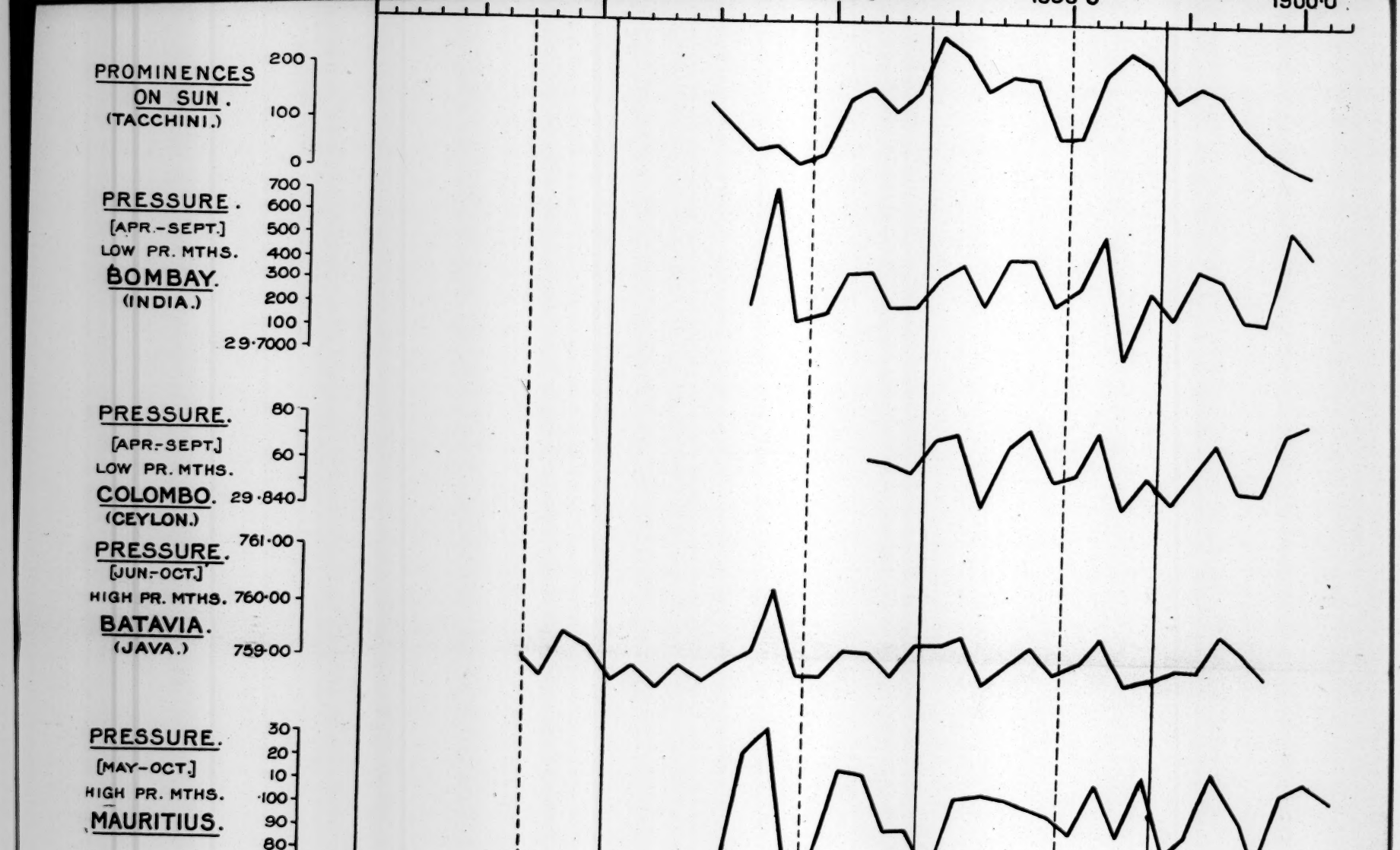
The striking similarity between these curves shows that over the whole of this area, which includes both north and south latitudes, the same kind of variations is in action, and that therefore the whole region is intimately connected meteorologically.

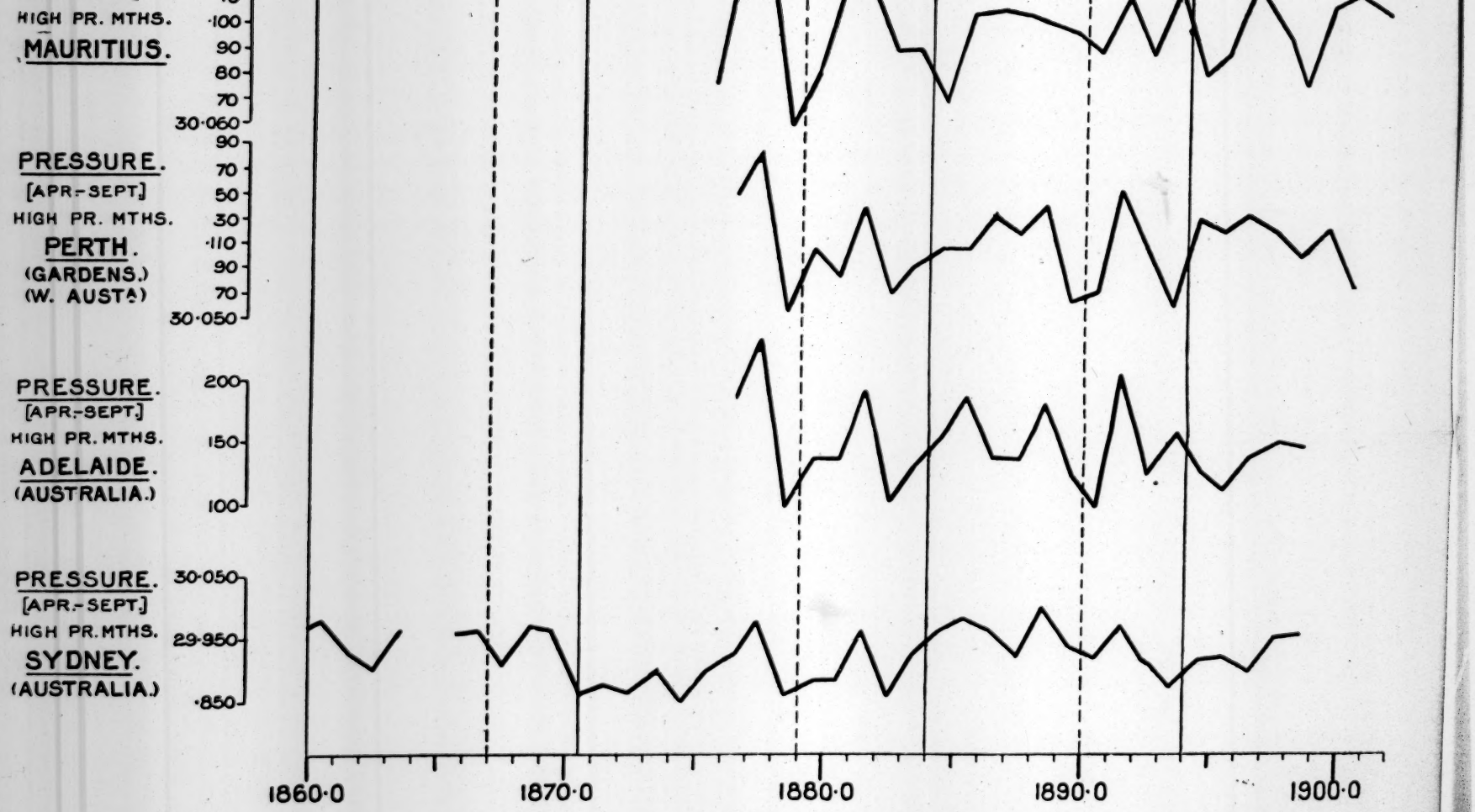
It was indicated in our previous paper that the pressure of Cordoba, in South America, was the inverse of that of India for the same period. Since the Indian pressure variations are seen now to extend over a

* "On Some Phenomena which suggest a Short-Period of Solar and Meteorological Changes." 'Roy. Soc. Proc.,' vol. 70, p. 500.

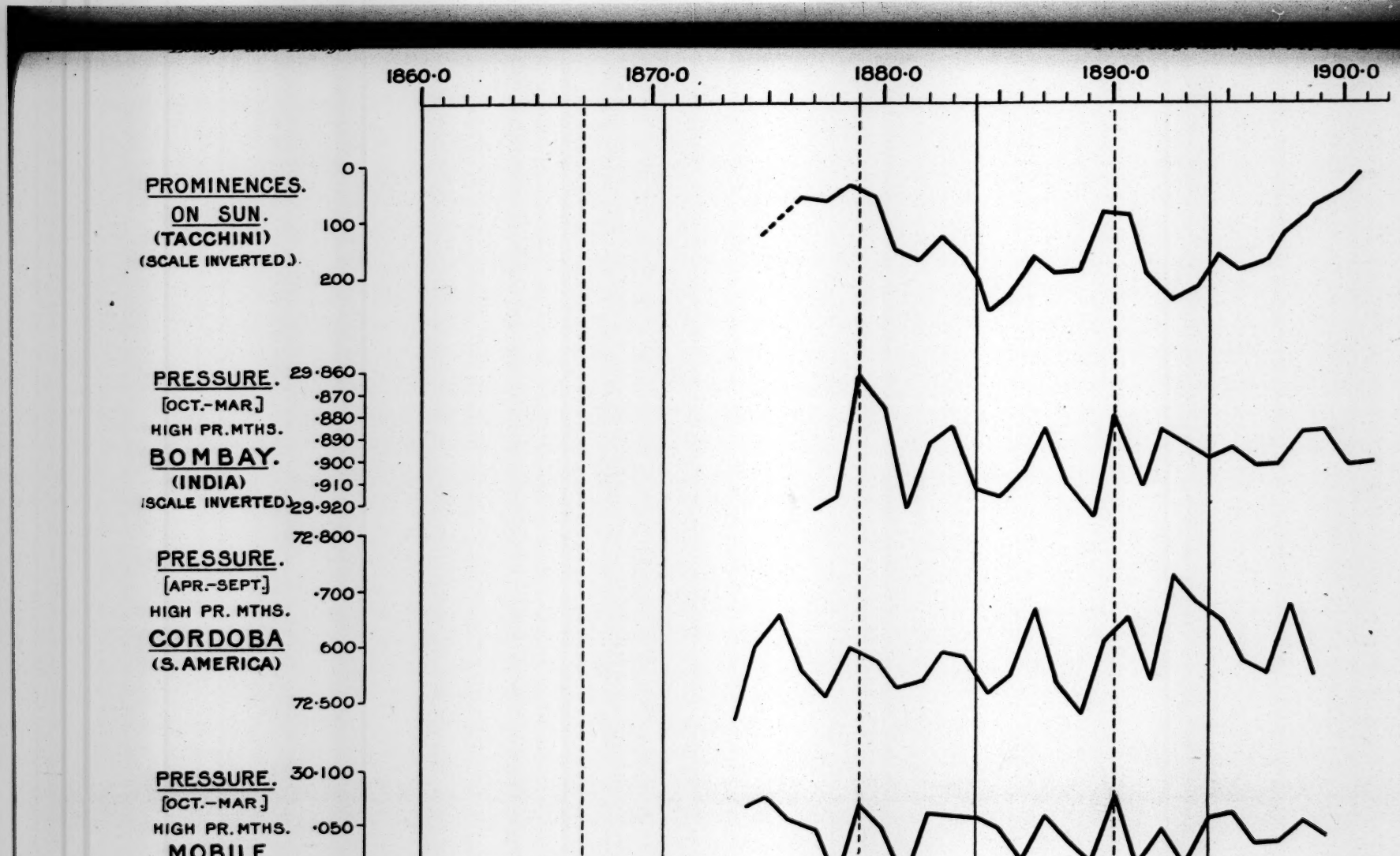
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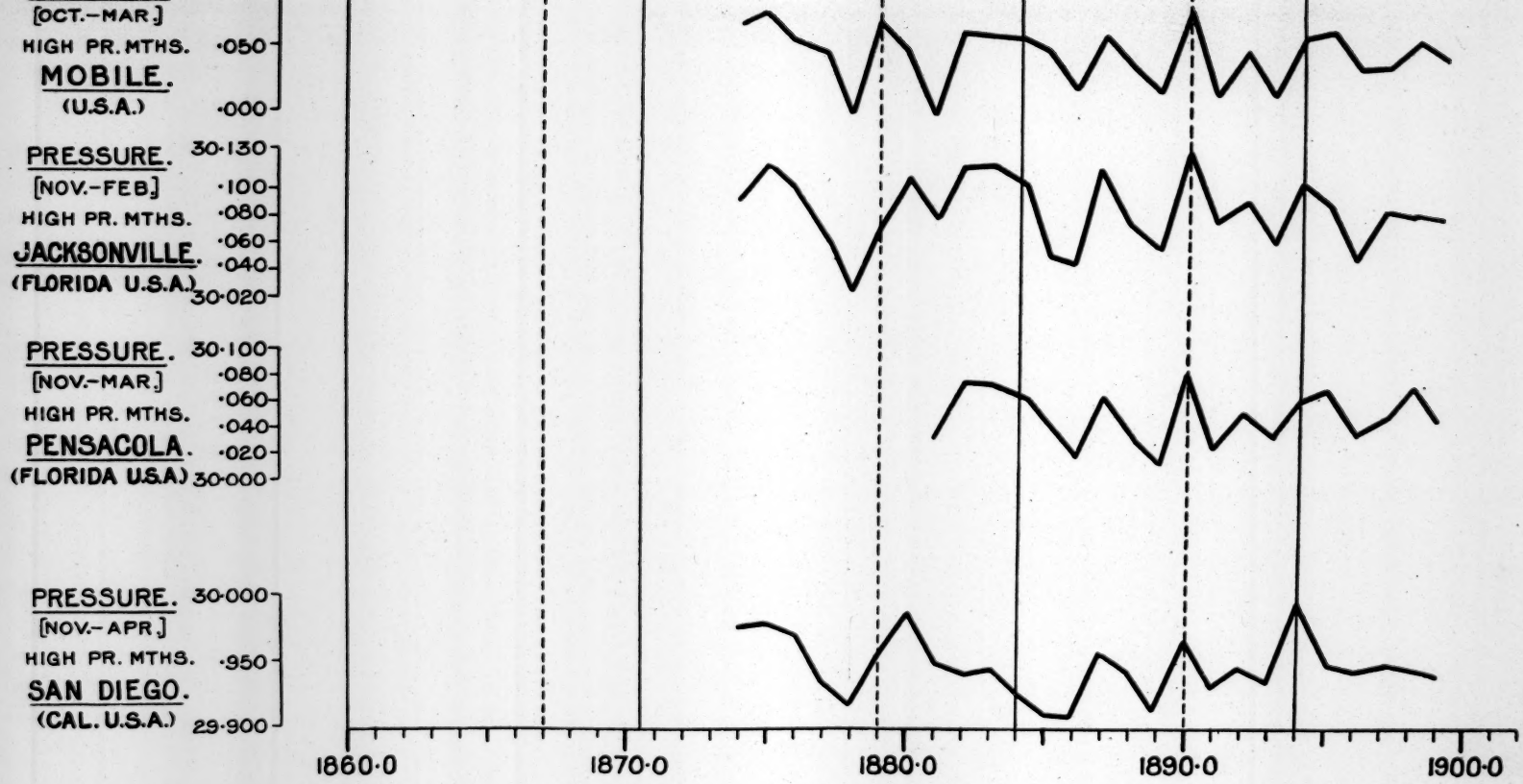



1900·0
PROMINENCES ON SUN. (TACCHINI.)
200
100
0
PRESSURE. [APR.-SEPT.] LOW PR. MTHS. BOMBAY. (INDIA.)
700
600
500
400
300
200
100
29·7000
PRESSURE. [APR.-SEPT.] LOW PR. MTHS. COLOMBO. (CEYLON.)
80
60
29·840
PRESSURE. [JUN.-OCT.] HIGH PR. MTHS. BATAVIA. (JAVA.)
761·00
760·00
759·00
PRESSURE. [MAY-OCT.] HIGH PR. MTHS. MAURITIUS.
30
20
10
·100
90
80



Note.—The vertical continuous and broken lines represent the epochs of maxima and minima of sunspot activity.


1860·0
1870·0
1880·0
1890·0
1900·0
PROMINENCES.
ON SUN.
(TACCHINI)
(SCALE INVERTED.)
0
100
200
PRESSURE.
[OCT.-MAR.]
HIGH PR. MTHS.
BOMBAY.
(INDIA)
(SCALE INVERTED.)
29·860
·870
·880
·890
·900
·910
29·920
PRESSURE.
[APR.-SEPT.]
HIGH PR. MTHS.
CORDOBA
(S. AMERICA)
72·800
·700
600
72·500
PRESSURE.
[OCT.-MAR.]
HIGH PR. MTHS.
30·100
·050



Note.—The vertical continuous and broken lines represent the epochs of maxima and minima of sunspot activity.

region on both sides of the equator, it was important to study the extent of the region in the New World in which pressure variations similar to those of Cordoba had been recorded.

As Cordoba represents an area south of the equator, a portion of the United States of America was taken as typifying an area with north latitude and in about the same longitude, and a commencement was made along the lowest available parallel of latitude. This was rendered possible by the kindness of Professor F. Bigelow, of the Weather Bureau, who very generously forwarded proof-sheets of a new reduction of the pressures of many stations. We wish to take this opportunity of expressing to him our best thanks.

Treating these pressures in the same way as those formerly investigated in the Indian region, several stations which had the best record were chosen. A graphical representation of the variations of four of these stations (Mobile, Alabama; Jacksonville and Pensacola, Florida; San Diego, California) is given in Plate 2, and for the sake of comparison the pressure of Cordoba, with the inverted curves representing the Bombay pressure and solar prominence variation. This series of curves refers in all cases to the variations of the means of the high pressure (winter) months (October to March in most cases). At Cordoba, which has a southern latitude, the high pressure months extend from April to September.

The result of the comparison shows that in this region of the world we have a large area, the pressure variations of which are strikingly similar to, but are the inverse of, those recorded in nearly the antipodal part of the globe.

The facts observed are so suggestive that we are continuing the inquiry by collecting and discussing observations made in other areas.

Although the general agreement between the two main sets of curves is most striking, there are minor differences which probably, as stated in the previous paper, will eventually help us to determine those cases in which the prominence effects on pressure are masked by some special local conditions. It may be added that the available observations of prominences refer more directly to their quantity than to their intensity.

We wish to express our thanks to Mr. W. N. Shaw, F.R.S., who kindly placed the records of the Meteorological Office at our disposal; and to Mr. Hodgson, who has extracted the requisite data from the available records of pressure, and constructed some of the curves.

"On the Vibrations and Stability of a Gravitating Planet." By J. H. JEANS, B.A., Isaac Newton Student and Fellow of Trinity College, Cambridge. Communicated by Professor G. H. DARWIN, F.R.S. Received November 8,—Read December 4, 1902.

(Abstract.)

The first part of the paper deals with the vibrations and stability of a gravitating elastic sphere. The matter is not necessarily homogeneous, but is arranged in spherical layers. It is pointed out that, in the classical investigation of the displacements produced in a gravitating sphere by given surface-forces, the most important of the gravitational terms is omitted. The effect of this omission is to necessitate a correction, and this may entirely invalidate the solution when we are dealing with spheres of the size of the earth or other planets. In fact, it appears that for a gravitating solid of the kind we are discussing the spherical configuration may be one of *unstable equilibrium*, the instability being brought about by the gravitational terms in the manner already explained in a former paper.*

Let a be the radius of the sphere, and γ be the gravitation constant; let ρ_0 be the mean density, and λ_0 the mean value of λ, one of the elastic constants. A general argument shows that the spherical configuration will be stable or unstable according as

$$\frac{\gamma\rho_0^2 a^2}{\lambda_0} < \text{or} > \phi \qquad \text{(i)},$$

where ϕ is a pure number which must be comparable with unity.

If we put in an artificial field of force we can imagine a spherical configuration of equilibrium in which the density and elastic constants have uniform values throughout. The artificial field of force is, of course, equal and opposite to the gravitational field produced by the matter of the solid. The stability or instability is determined by a criterion of the form of (i), and ϕ can now be calculated exactly. If we suppose λ_0 to represent $\lambda + 2\mu$ in Love's notation ($m + n$ in that of Thomson and Tait), we find the values

$$\phi = 1{\cdot}6, \text{ when } \mu = 0,$$

$$\phi = 1{\cdot}3, \text{ when } \mu = \lambda.$$

The vibration through which instability first enters is one in which the displacement at every point is proportional to a harmonic of the *first* order. It appears probable that for spheres which are not homo-

* "The Stability of a Spherical Nebula," 'Phil. Trans.,' A, vol. 199, p. 1.

geneous, but in which the density is greatest near the centre, the values of ϕ will be greater than those just stated, but the critical vibration will still be such that the displacement is proportional to a first harmonic term.

In the former paper, already referred to, the suggestion was put forward that the instability of a nebular sun or planet, which, upon the nebular hypothesis, is supposed ultimately to result in the ejection of a satellite, may be largely brought about by a gravitational tendency to instability of the kind just described. We take, for the moment, an extreme hypothesis, and imagine that this agency is the preponderating agency, and that the rotational tendency to instability may be disregarded in comparison. We then find that when the ejection of a satellite is taking place, or has just been completed, the value of $\gamma\rho_0{}^2\alpha^2/\lambda_0$ must be nearly equal to ϕ.

Except for the changes which have occurred since the consolidation of the planets, the solar system supplies material for testing this conclusion. When a number of planets of varying masses have thrown off satellites, we find (upon our present extreme hypothesis) that the masses ought to be proportional to the *squares* of the radii. It is found that this law is approximately obeyed in the solar system. It is further found that the absolute values of the masses and radii are approximately such as would be expected.

It is interesting to compare two extreme hypotheses, the first referring the phenomena of planetary evolution solely to rotational, the second solely to gravitational, instability. Given the approximate values of the density and elasticity of a planet, and the fact that this planet has thrown off a satellite; then the former hypothesis leads to a certain inference as to the angular momentum of the system, the latter to an inference as to the radius of the primary. The former leads to no inference at all as to the size of planets which are to be expected—they are as likely to be of the size of billiard-balls as of the size of the planets of our system—while the latter leads to no inference as to the angular momentum of the system, but presupposes it to be small. The contention of the present paper is that the inferences which are drawn from the former hypothesis are not borne out by observation on the planets of our system, while those which are drawn from the latter are borne out as closely as could be expected. The true hypothesis must of necessity lie somewhere between the two extremes which we are comparing, but the evidence seems to show that it is much nearer to the latter (gravitational) than to the former (rotational).

We next consider a number of questions connected with the figure of the earth. It seems to be almost certain that the present elastic constants of the earth are such that a state of spherical symmetry would be one of stable equilibrium. On the other hand, if we look

backwards through the history of our planet, we probably come to a time when the rigidity was so small that the stable configuration of equilibrium would be unsymmetrical. At this time the earth would be pear-shaped, and the transition to the present approximately spherical form would take place through a series of ruptures. It is suggested that the earth, in spite of this series of ruptures, still retains traces of a pear-shaped configuration. Such a configuration would possess a single axis of symmetry, and this, it is suggested, is an axis which meets the earth's surface somewhere in the neighbourhood of England (or possibly some hundreds of miles to the south-west of England). Starting from England, we find that England is at the centre of a hemisphere which is practically all land: this would be the blunt end of our pear. Bounding the hemisphere we have a great circle of which England is the pole, and it is over this circle that earthquakes and volcanoes are of most frequent occurrence. Now, if we suppose our pear contracting to a spherical shape, we notice that it would probably be in the neighbourhood of its equator that the changes in curvature and the relative displacements would be greatest, and hence we should expect to find earthquakes and volcanoes in greatest numbers near to this circle. Passing still further from England we come to a great region of deep seas—the Pacific, South Atlantic, and Indian oceans: these may mark the place where the "waist" of the pear occurred. Lastly, we come almost at the antipodes of England to the Australian continent: this may mark the remains of the stalk-end of the pear.

"Experiments on the Effect of Mineral Starvation on the Parasitism of the Uredine Fungus, *Puccinia dispersa*, on species of *Bromus*." By H. MARSHALL WARD, Sc.D., F.R.S., Professor of Botany in the University of Cambridge. Received November 4,—Read November 27, 1902.

I have shown in previous publications that the parasitic Uredine *Puccinia dispersa*, growing on grasses of the genus *Bromus*, is usually very closely adapted to the species of host-plant selected: that although no morphological differences can be detected between the fungus as met with on different species of *Bromus*—A, B, C, &c.—it by no means follows that spores from the parasite, as found growing on A, will infect B or C, or that spores from the fungus as reared on B or C will infect the species A.

On the whole, it has so far appeared probable that the fungus growing on a given species—*e.g.*, *B. mollis*—infects most readily those

species which are most nearly related to *B. mollis*, less and less readily species more remote from *B. mollis*, but in the same sub-genus, and least readily, or not at all, species in other sub-genera.

In the attempt to obtain some insight into what causes are at the bottom of this remarkable phenomenon—the predisposition to, or immunity from, infection of the host by the parasite—it was shown that anatomical differences on the part of the host-plants, such as the sizes and numbers of stomata, hairs, and so forth, do not suffice to explain it, since no relation could be detected between the curves expressing the percentage of infection and those expressing the sizes, numbers, &c., of the hairs, stomata, &c.

On the other hand, the evidence suggested some such assumption as the following. The fungus, when growing on a species of *Bromus* A, may refuse to infect another species B, either because B secretes some body of the nature of an enzyme or anti-toxin which effectually keeps the mycelium of the fungus at bay, or because the fungus habituated to the peculiar nutritive or other conditions afforded it by the host-plant A, cannot immediately adapt itself to the very different conditions offered by the species B.

Although the attempts to isolate any such anti-toxin failed, and experiments of a preliminary character to test the effect of differences of nutrition yielded little or nothing of a positive nature, I showed in the discussion of the probable factors concerned that some subtle relations between host and parasite must be assumed to account for the curious facts of immunity and predisposition on the part of the former, and of capacity and incapacity for infection on the part of the latter, in each case in various degrees according to the species of host offered for infection, or on which the fungus has hitherto been reared.

During the past year I have attempted to pursue this subject further, and limit myself for the present to the following theme. If the varying infective power of the fungus towards different species of host-plant is derived solely from the "nutritive conditions" afforded it by the host-plant it has hitherto been growing upon, two cases are possible—(1) these "nutritive conditions" may be simply the expression of the power of the tissues to yield certain food-substances to the parasite in proper proportions and in sufficient quantity, or (2) they may imply some more subtle relations between the mycelium of the fungus and the living contents of the host-cells. For instance, it may be not sufficient that the food-substances suitable to the fungus should exist in the cells of the host, but they must be there in a certain superabundance, or presented in a certain manner, and so on; or, it may be that the fungus must be vigorous up to a certain standard before it can obtain a hold on such food, and so on.

In order to test some of the possibilities here referred to I planned

experiments to see whether starving the host-plant of one or other of its necessary food-materials would (1) affect its predisposition to infection, or (2) affect the capacity for infection of the fungus grown on the starved plants, or (3) in any other way affect the fungus or its host.

On July 7, 1902, fourteen beakers were selected and filled with equal quantities of a clean coarse-grey sand, carefully washed and dried, but still containing traces of ingredients which the root-hairs of such grasses as the Bromes are capable of selecting. This was not to be avoided or regretted, since my object was not to attempt to grow seedlings totally deprived of necessary salts, resulting in their premature death, but to bring up plants so starved of certain such ingredients that while they could go on living long enough for the purposes of the experiment, they would nevertheless exhibit the effects of the deficiency, and possibly re-act on the parasite.

The beakers of sand were lettered A to O, and treated as follows:—The beaker A received 200 c.c. of distilled water only, so that the only mineral supplies available to the seedlings—after exhausting the traces in the endosperm—would be such as the root-hairs could dissolve from the sand grains. Another beaker received an equal quantity of a cold water-extract of fresh horse-dung, representing a liquid of high manurial value. A third beaker received an equal quantity of a normal nutritive mineral solution containing nitrates, phosphates, and sulphates of potassium, calcium and magnesium; and a fourth the same with the addition of five drops of a dilute solution of ferric chloric.

One each of the remaining beakers received a similar solution of the nutritive salts, but in each case with the omission of *one* element, viz.: calcium, magnesium, nitrogen, phosphorous, or potassium; while the other beaker of each pair received the same, together with traces of iron salt.

By these means I had prepared a soil in each beaker which was of suitable consistency for growing such sand-loving grasses as Bromes, but which was in each case deficient in one or other of the necessary ingredients for normal nutrition—except in so far as I added such ingredients—but in no case absolutely devoid of these necessary salts, as otherwise the seedlings could not be expected to live long enough for the purposes of the experiment.

On July 8, having allowed the solutions to soak completely into the sand, seven grains of *Bromus secalinus*, carefully cleaned and selected, were sown in each beaker, and the whole left under large bell-jars to germinate in a suitably lighted position in the laboratory. Germination followed in due course, though somewhat slowly, as the sand was rather wet, and on July 15, from two to five seedlings about 30—50 mm. high were showing above ground in each beaker except

one, and the whole were now placed in a sheltered situation in the open, the bell-jars being raised on large blocks so that their rims just covered the rims of the beakers; thus permitting a free circulation of air, but protecting the whole from beating winds or rain. An awning was also provided, and carefully adjusted during hot days as necessary. Growth proceeded fairly rapidly, though of course some retardation was inevitable owing to the impervious nature of the beakers as contrasted with pots having porous sides, and by July 18 each beaker had from two to seven satisfactory seedlings.

On July 23, ten of the beakers had from five to seven excellent seedlings each: three had but four each, and one had only two successful seedlings. So far there was but little difference observable between the different seedlings, each of which was unfolding its third leaf. Nevertheless, there was evidence that the stores of food-materials in the indosperm were now being exhausted, and that the root-system was beginning to feel the effects of the differences in mineral supplies. Consequently, I fished out the remains of dead or un-germinated grains, and proceeded to infect certain of the seedlings in each beaker with Uredo-spores of *Puccinia dispersa* obtained from *Bromus squarrosus*. This species was selected because I had an abundant stock of vigorous spores at hand, and had already satisfied myself that they readily infect *Bromus secalinus*.

The infection was effected by placing equal doses of the fresh spores on each leaf chosen; the bell-jars were then lowered in order to keep the plants in a moist atmosphere for 24 hours, and then raised again on blocks for another 24 hours, after which the bell-jars were removed each morning, and only replaced at night or during rain.

The results of infection were already evident on one or two plants on July 31 and August 1, increasingly so on August 3, and by August 5 the pustules were as prominent as they ever became during the period of the experiment—*i.e.*, up to August 29—except in so far as some of the larger ones ran together or produced more spores.

The results are summarised in Table I, but it should be noted that it has been impossible to compress into the table the details of the observations from day to day as to the stature, robustness, colour, number and breadth of leaves and so on, or as to the rate of development of the plants and of the flecks and pustules of the parasite.

In order that the reader may gather an impression of the appearance of these seedlings during the progress of the experiment, however, I append photographs* of one set of the beakers in fig. 1, and of a representative series of the plants themselves, carefully extracted

* I have to thank my son, F. K. Ward, for the preparation of these photographs.

from the loose sand in water and the roots washed and displayed on black paper (fig. 2); while in fig. 3 are appended other representative specimens, similarly carefully extracted and washed and displayed with the roots on black paper, and the shoots on white paper.

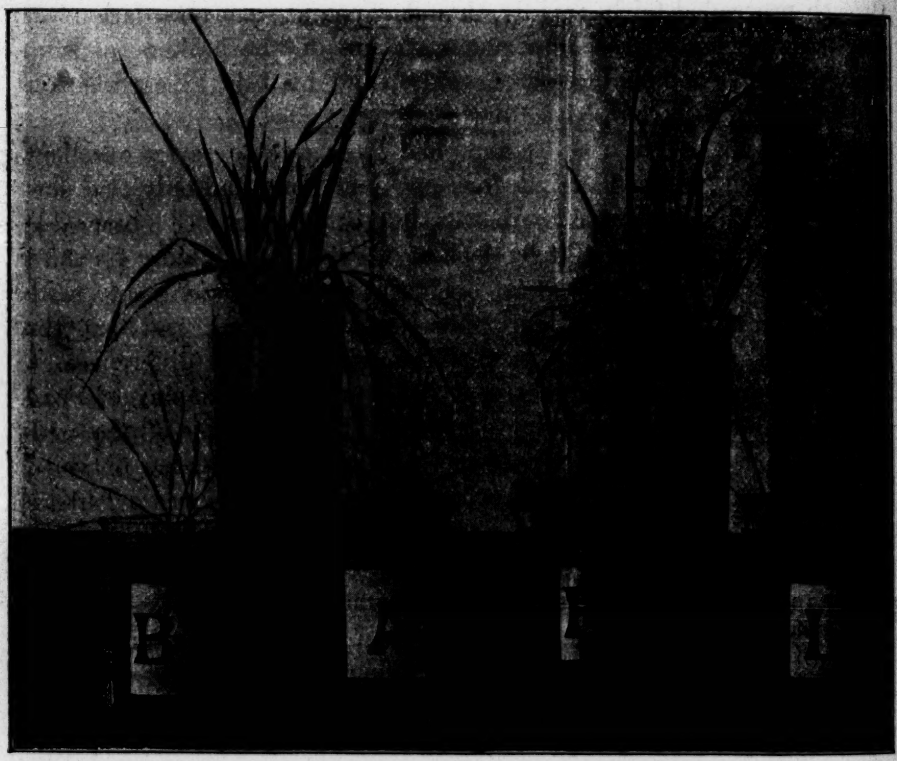


FIG. 1.—Specimen from Experiment 47 (Table I), photographed 41 days after sowing, showing differences in stature and breadth of leaf. The plants in A had received distilled water only; in B nutritive solution minus phosphorus; in E no nitrogen; in K no magnesia, and with five drops of ferric chloride; in N normal mineral solution with iron salts. L had no nitrogen, but five drops of iron solution.

The facts recorded in this table and in the legend to these figures speak for themselves, but the following details may be noted.

The effects of the deficiency of all salts (A) of phosphorus (B) and of nitrogen (E) are distinctly observable in the poor development of the roots, as well as of the feeble and narrow leaves; but although the opposite effects of the rich manurial action of the horse-dung decoction (O) and the normal mineral solution (G) are plain, the differential results of the solutions minus calcium and potassium are by no means obvious, nor did the addition of the five drops of dilute ferric chloride seem to make much difference.

Apart from an error in regard to the calcium (*c*, Table I), I attribute this to the circumstance that it is extremely difficult to starve such

psammophilous plants as the Bromes of the minute quantities of these elements needed for their growth; the root-hairs appear to be able to

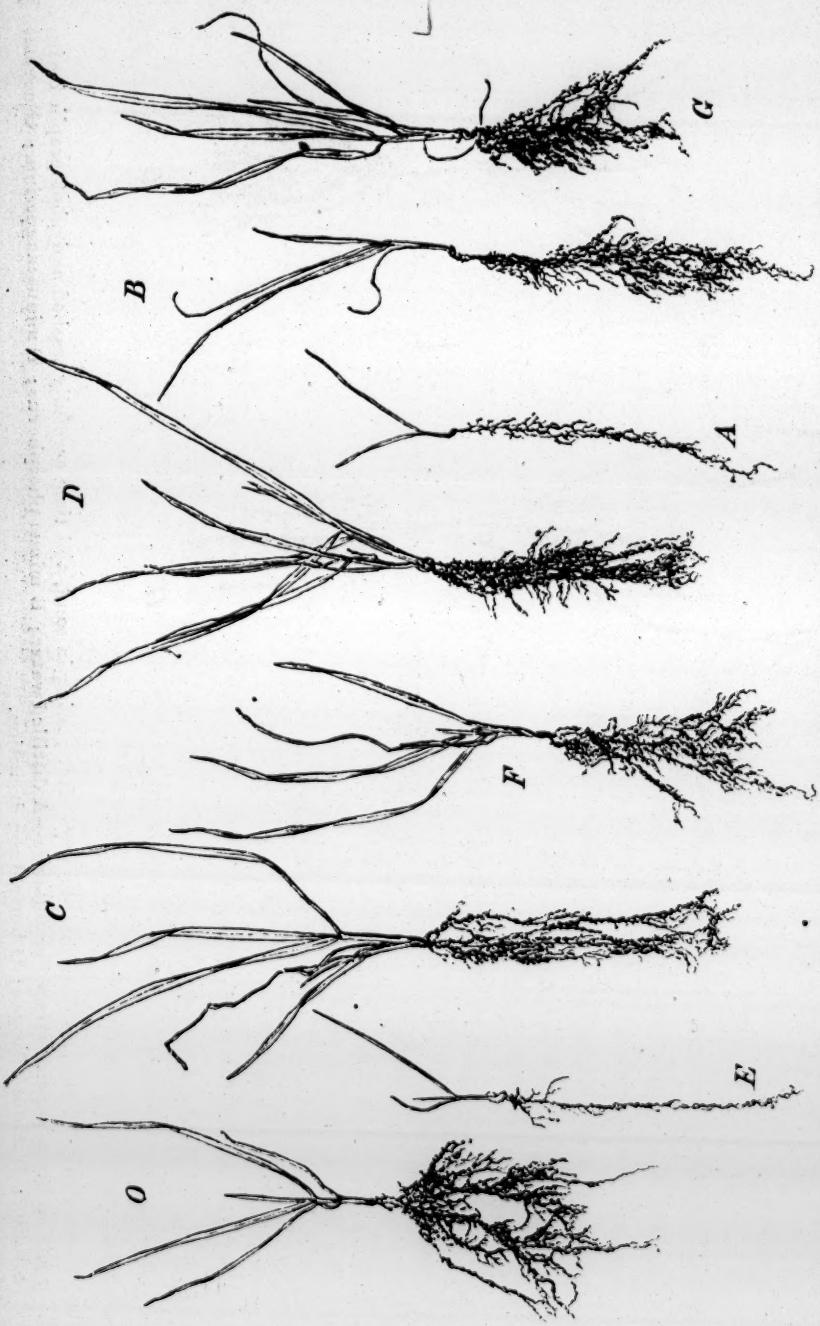


FIG. 2.—Plants of Experiment 47 removed from the sand and carefully washed, showing the development of root and shoot after 44 days. Treated as follows:—A distilled water; B minus phosphorus; C minus calcium; D minus magnesium; E minus nitrogen; F minus potassium; G normal solution; O horse-dung decoction.

obtain traces of these substances from the grains of sand, and it would be necessary to repeat the experiments with some other medium—perhaps precipitated silica or pure quartz—to obtain through starva-

tion of these elements, a result not wished for in the present connection.

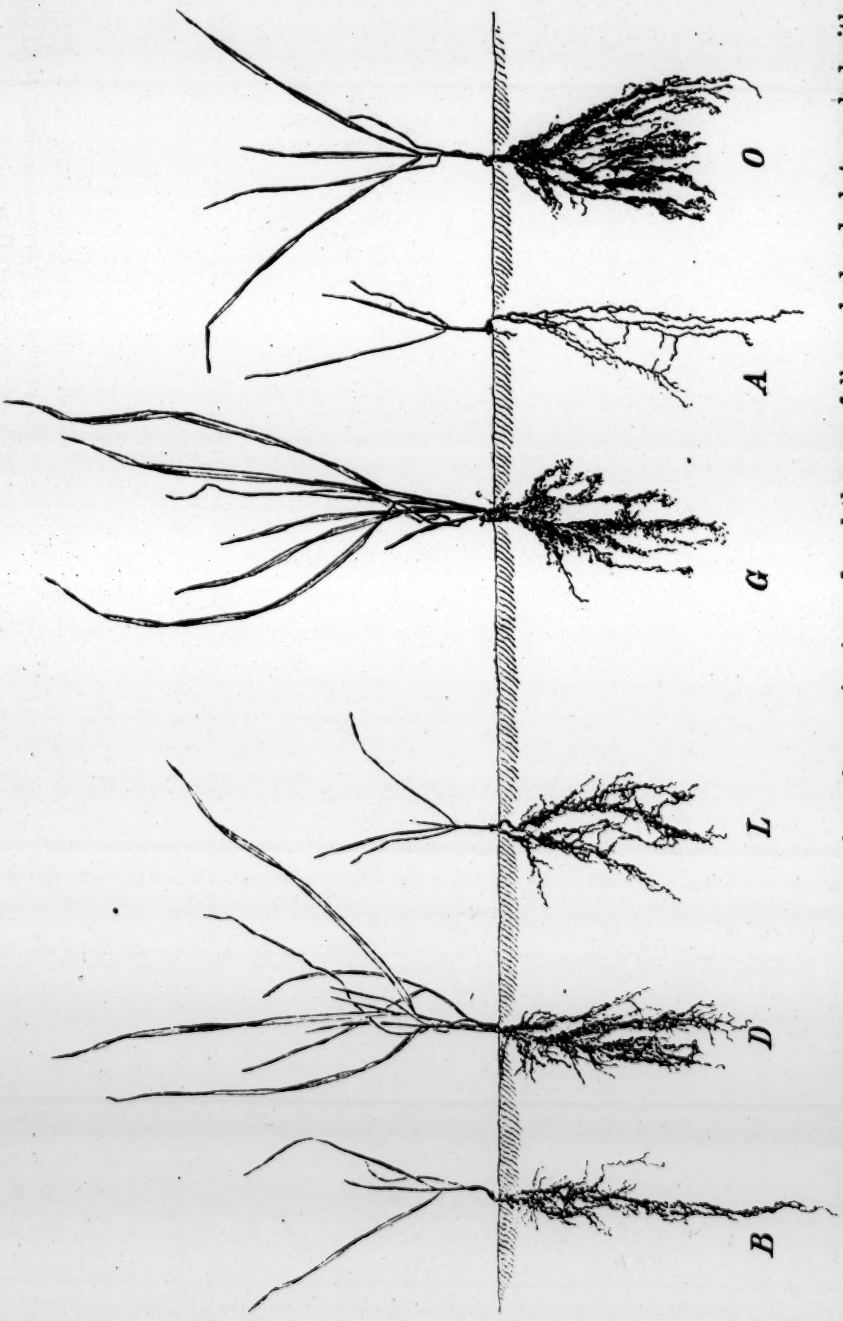


FIG. 3.—Seedlings of *Bromus secalinus* 44 days after sowing in sand, and then carefully washed and photographed with roots on black paper. Treated as follows:—A distilled water; B minus phosphorus; D minus magnesium; G normal solution; L minus nitrogen; O horse-dung decoction.

But although the differences in size were not obvious in these cases, the magnesium-starved seedlings exhibited some effects which can only have been due to lack of supplies, viz.: the tips of the leaves

solution; L minus nitrogen; O horse-dung decoction.

Table I.—Exp. 47. Seedlings of *Bromus secalinus*, sown in coarse cleaned

Exp. No.	Treatment.	Number of seedlings.	Number inoculated.	Condition c	
				Stature.	Robustness
47, *a*	Distilled water.....	7	4	10—12 cm.	Small
b	Minus phosphorus	4	3	12—16 „	Medium
*c**	„ calcium	5	3	20—22 „	Stout
d	„ magnesium	5	3	One = 9 cm., others = 16—18 cm.	Slight
e	„ nitrogen	5	3	Two = 5— 6 cm. Two = 14—15 „ One = 1 cm.	Slight
f	„ potassium	5	4	16—18 cm.	Stout
g	Normal.....	4	3	19—21 cm.	Stout
h	P-free + Fe	7	5	12—16 „	Medium
i	Ca-free + Fe	7	5	20—22 „	Stout
k	Mg-free + Fe.....	6	4	16—18 „	Slight
l	N-free + Fe	6	4	10—12 „	Small
m	K-free + Fe.....	7	4	16—18 „	Stout
n	Normal + Fe	5	3	19—21 „	Large
o	Horse-dung	6	6	16—17 „	Stout and coarse.

* On July 18 a dose of the K-free solution was given to this by mistake, consequently it re

cleaned Sand in Beakers, to which doses of Nutritive Salts were added on July 7, 1902.

ndition of plants.			Results on July 31.	Results on August 1.	Results on August 3.	Numb seedl infe
obustness.	Colour.	Average number of leaves.				
Small	Rather pale	2 narrow	0	Flecks?	One second leaf shows pustules	1
Medium	Slightly pale	2½ narrow	0	0	0	0
Stout	Good green	3 broad	0	0	One second leaf shows pustules	1
Slight	Fair, but tips of leaves dying off.	2½ medium	0	0	One first and two second leaves show pustules	3
Slight	Pale	2—3 narrow	0	0	0	1
Stout	Good green	3 medium	Flecks?	One first leaf and three second leaves have pustules	Two first and three second leaves pustuled	4
Stout	Bright green	3½ broad	Flecks?	One first and one second leaf have pustules	Three first and two second leaves show pustules	3
Medium	Slightly pale	2½ narrow	0	0	0	4
Stout	Vivid green	3 broad	Flecks?	Two first leaves and four second leaves pustuled	Three first and five second leaves show pustules	5
Slight	Good green but tips dying off	2½ rather narrow	0	Flecks?	Two second leaves pustuled	3
Small	Good green	2—3 narrow	0	Flecks?	Three first and three second leaves pustuled	3
Stout	Good green	3 medium	Flecks?	Flecks?	One first and three second leaves pustuled	4
Large	Deep green	3 broad	Flecks?	Two first leaves pustuled	Three first leaves show pustules	3
Stout and coarse.	Good green	3 broad	Flecks?	Three first leaves and one second leaf pustuled	Five first and three second leaves have pustules	6

tly it received all salts, but in different proportion from the normal. † On August 23 spores were ta

‡ First leaf dead in one plant also.

o which doses of Nutritive Salts were added on July 7, 1902. On July 23, infected

Average number of leaves.	Results on July 31.	Results on August 1.	Results on August 3.	Results on A	
				Number of seedlings infected.	Number infected on 1st leaf.
2 narrow	0	Flecks?	One second leaf shows pustules	1	0
2½ narrow	0	0	0	0	0
3 broad	0	0	One second leaf shows pustules	1	0
2½ medium	0	0	One first and two second leaves show pustules	2	1
2—3 narrow	0	0	0	1	1
3 medium	Flecks?	One first leaf and three second leaves have pustules	Two first and three second leaves pustuled	4	4
3½ broad	Flecks?	One first and one second leaf have pustules	Three first and two second leaves show pustules	3	3
2½ narrow	0	0	0	4	3
3 broad	Flecks?	Two first leaves and four second leaves pustuled	Three first and five second leaves show pustules	5	4‡
2½ rather narrow	0	Flecks?	Two second leaves pustuled	3	2
2—3 narrow	0	Flecks?	Three first and three second leaves pustuled	3	3
3 medium	Flecks?	Flecks?	One first and three second leaves pustuled	4	2
3 broad	Flecks?	Two first leaves pustuled	Three first leaves show pustules	3	2
3 broad	Flecks?	Three first leaves and one second leaf pustuled	Five first and three second leaves have pustules	6	5

t in different proportion from the normal. † On August 23 spores were taken from these plants and
‡ First leaf dead in one plant also.

nfected 1st and 2nd Leaves with Spores from *Bromus squarrosus*.

ults on August 5.			
ıber ed on eaf.	Number infected on 2nd leaf.	Number infected on both 1st and 2nd leaf.	Remarks as to condition at end of experiment.†
	1	0	Two or three small patches of small pustules, yielding fair spores. Leaf pale in infected area.
	0	0	No trace anywhere of pustules.
	1	0	Only one small group of small pustules, rather better than in *a*. Leaves green.
	2	1	Small but fairly well-developed pustules of spores on one plant; only one pustle on the other. Leaves very pale and tips dead.
	0	0	One very minute pustule on pale first leaf.
	4	4	Extensive infection, and several patches show strong pustules. Leaves rich green.
	3	3	Excellent crop of spores on large pustules, and leaves rich green.
	3	2	Minute pustules, difficult to detect and bearing but few spores. Colour fair.
‡	5	4	Very good pustules with abundant spores, but few and small areas or patches of infection. Leaves good green.
	3	2	Medium-sized patches of small pustules with not many spores. Pale, and tips dead.
	3	3	Attacked area pale. Pustules small and few, and yield few spores.
	3	1	Few but medium-sized pustules. One leaf has a large patch. Green.
	3	2	Very large areas of well-developed pustules, rich in spores. Leaves deep green.
	6	5	All pustules large and rich in spores, but areas of leaf around pale; otherwise deep green.

ants and infections made on test-tube cultures of *Bromus secalinus* (see Table II).

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d green	3 broad
, but tips leaves ying off.	2½ medium
	2—3 narrow
d green	3 medium
ght green	3½ broad
htly pale	2½ narrow
d green	3 broad
d green t tips ing off d green	2½ rather narrow 2—3 narrow
d green	3 medium
p green	3 broad
d green	3 broad

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were apt to show early yellowing and withering, as well as some degree of stunting.

When we come to examine the results of infection, it is clear that no great differences were observable between the various seedlings. The first flecks—*i.e.*, pale patches indicating the presence of the fungus in the tissues—were visible on July 31 and August 1, that is to say, on the eighth or ninth day after infection, and by August 3 pustules were developed on all the plants except those starved of phosphorus, while two days later even these were definitely pustuled in one of the two beakers (*h*, Table I), though the other remained free of them to the end (*b*, Table I). As will be seen from further experiments, however, we can lay no stress on this latter case, and must conclude that:—*Lack of minerals in no way secured immunity from infection, though seedlings deficient in phosphorus or in nitrogen tended to show retardation of infection.*

When we look into the matter from another point of view, however, the conclusion seems inevitable that there are considerable differences in the *quantity* of fungus mycelium, and consequently of Uredo-spores, developed from the latter, which the various seedlings were able to support.

Taking the extreme cases, and comparing the sizes of the infected areas on the phosphorus-free plants (*h*, Table I) and those on the plants supplied with normal mineral solution (*g* and *n*, Table I), the latter were found to be much the larger, and similarly with the pustules themselves: the numerous large well-developed pustules on *g* and *n* bore many hundred times as many spores as did the few small pustules on *f*, and similar results were observable in less marked degrees on others. It must therefore be concluded that:—*Mineral starvation makes itself felt quantitatively in the number of uredospores which can be produced by the fungus in the tissues of the starved leaves.*

Now arose an important question:—Are the spores on the small starved pustules in any way different from those produced by the large well-developed pustules of normal or richly-manured plants? For instance, can any differences in size, colour, or marking, or in capacity for germination, be detected in spores from the small and few pustules of *a*, *e*, *h*, or *l* (Table I), and those from the large well-developed pustules of *g*, *n*, or *o*?

As regards the morphological features, direct examination showed that no differences were to be detected; the properly ripened spores in all cases were normal.

As regards their capacity for germination, I proved by sowing them in separate watch-glasses of distilled water that in every case *the pustules yielded spores capable of normal germination, and that in proportions which showed no relation to the degree or kind of starvation of the seedling which bore them* (see Table II, col. 4).

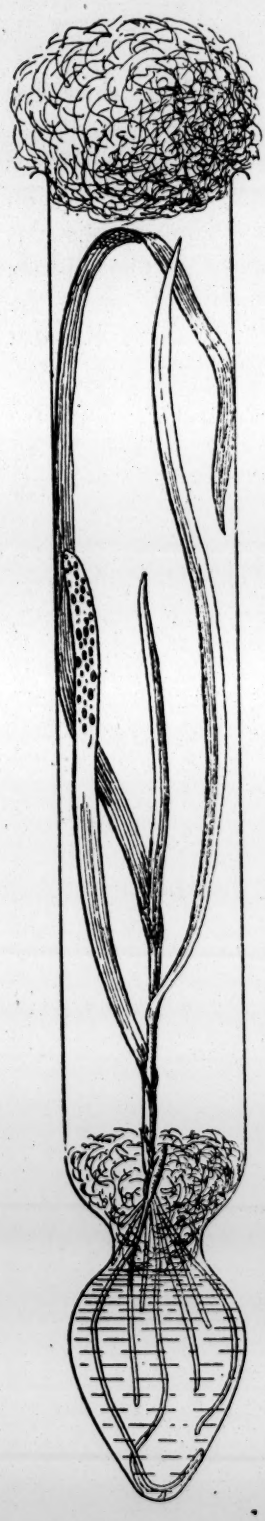


FIG. 4.—Tube culture of Brome-seedling arranged as a test-plant for infection, as described on p. 146.

It now remained to test the further question :—Are such spores equally capable of infecting other seedlings?

In order to test the capacity for infection, or virulence, of the spores developed on the experimental plants, I employed tube-cultures of seedlings as described in a previous communication,* with a slight modification in detail, as follows (see fig. 4) :—

Picked grains of *Bromus secalinus* were sown singly in test-tubes of sand, to which the normal mineral solution was added, and sterilised. When the seedlings were well up, and nine days old, on August 14, each having one good green leaf, a sowing was made on each from the uredo pustules on one of the plants of Experiment 47 (Table I), as shown in the annexed tabular summary (p. 147).

It will be seen that in every case the results of infection were positive. Those spores which had been reared on seedlings starved of phosphorus (*h*), and on those starved of all salts (*a*), were behind the others in their rapidity of effect; but we cannot lay much stress on this, because the same was true of spores reared on what should have been the calcium-starved plants (*c*), which accidentally received a dose of calcium by mistake. On the other hand, the most vigorous spores were those reared on plants to which horse-dung decoction had been added.

I am not disposed to lay stress on these latter points, however, but am content at present to regard the positive results as amounting to proof *that spores even when reared on starved seedlings are capable of normal germination and infection when placed on the leaves of other—normal—seedlings.*

A second experimental series of sand-cultures was started on July 10, 1902, in sixteen beakers, arranged as before, but with the following differences in detail. The sand used was a finer and whiter silver sand,

* "On Pure Cultures of a Uredine." 'Proc. Roy. Soc.,' vol. 59, p. 451.

1902.

Table II.—Exp. 60. Test-tubes of Sand + Normal Solution, each with a Seedling of *B. secalinus*, sown August 5.

Table II.—Exp. 60. Test-tubes of Sand + Normal Solution, each with a Seedling of *B. secalinus*, sown August 5. Infected August 14 with Spores of Exp. 47 (*B. secalinus*).

No. 60.	Plant whence spores came, and its treatment (see Table I).	Quantity and quality of infecting material obtained from Exp. 47.	Germination of spores used.	Results on Aug. 21.	Results on Aug. 23.	Results on Aug. 26.	Remarks.
a	47, *a*. Distilled water..	Pretty good batch	Good	Flecks..	+	+	Pustules just evident on August 22.
b	..	..	..	..	..	..	Control. No trace of infection.
c	47, *c*. No Ca*.....	One fair batch	Excellent.	..	Flecks..	+	Few but good pustules on August 26.
d	47, *d*. No Mg.....	Two fair batches	Good	+	+	+	Spores already forming on August 21.
e	47, *e*. No N	One very small batch	Fair	+	+	+	,, ,, ,,
f	47, *f*. No. K.....	Plenty and good.....	Excellent.	+	+	+	,, ,, ,,
g	47, *g*. Normal	Abundant and good.....	Splendid..	+	+	+	,, ,, ,,
h	47, *h*. P-free + Fe ..	Minute, and few spores†.	Very good	..	Flecks..	+	Few and small pustules, but evident on August 26.
i	47, *i*. Ca-free + Fe ..	Plenty and good	Excellent.	+	+	+	Spores evident on August 21.
k	47, *k*. Mg-free + Fe..	Two good batches	,,	+	+	+	,, ,,
l	47, *l*. N-free + Fe...	Two fair batches.....	,,	+	+	+	,, ,,
m	47, *m*. K-free + Fe ..	Two fair batches	Good	+	+	+	,, ,,
n	..	..	..	..	..	..	Control. No trace of infection.
o	47, *o*. Horse + Fe ...	Plenty and good.....	Very good	+	+	+	Most advanced of all; formed spores before any of the others on August 20.

* But note Ca added by error on July 18 (see Exp. 47).

† Three minute batches; too small to see when put on leaf.

chemically purified by acid and washing, and stronger doses of the mineral solution were employed. Each beaker received an equal weight of the sand (1500 gr.), and of soluton (300 c.c.), and 8 grains of a species of *Bromus*, received under the name of *B. pendulinus*, were sown in each. This species—which differs little, if at all, from *B. patulus*—has been thoroughly investigated with regard to its predisposition to attack by the uredo of *Puccinia dispersa*, and was chosen because it has proved to be the most sensitive to infection of all the forms I have in culture. The grains were hand-picked and clean, but were not specially sterilised in any way.

The beakers were treated as before, and on July 15 seedlings were showing in all but one of the beakers, and by the 18th each of thirteen had from seven to eight seedlings, two had four each, and one none. By July 30 every beaker had from six to eight excellent seedlings, except one, and this remained barren to the end, possibly owing to drowning of the grains, or too deep sowing.

By this date there were marked differences in stature, and some in colour, between the various pairs of sets; the smaller seedlings in the beakers deprived of all added salts, or deficient in nitrogen or in phosphorus being particularly noticeable. Not only were the leaves of these fewer and shorter but also narrower, and the whole plantlet looked feebler in each case than corresponding specimens in other beakers. Most of the latter had three or four leaves fully expanded, with the fourth or fifth just peeping through, but these feebler seedlings only showed two mature leaves and a third one just beginning to unfold, or three leaves with as yet no signs of the fourth.

On July 30 I infected, chiefly on the second leaf,* with Uredospores of the fungus grown on *Bromus patulus*, a form known from experiments to infect this species of seedling very readily. The infected plants were then placed under moist bell-jars, lifted in due course, and treated as before.

On August 8 several of the plants showed pustules, indicating successful infection, and by August 12 it was possible to estimate the degree of attack in each case, and to compare the relative virulence or vigour of the pustules and spores developed.

The essential facts are summarised in Table III.

Here, again, certain concordant facts come prominently to light. As before the infection is generally successful on just those plants, and at just those spots where the inoculation occurred. In the apparent exceptions (*e.g.*, in *g* and *i*, Table III), I have no reason to doubt that the extra-infection was due to the almost unavoidable spluttering of spores, or the contact of inoculated leaves with others, a particularly easily incurred danger with this extremely susceptible species.

* In several cases the first and third leaves also.

Table III.—Exp. No. 50. Seedlings of *B. pendulinus*, sown in fine cl

No. 50.	Treatment.	Number of plants in beaker.	Number inoculated.	Con	
				Stature.	Robustness.
a	Distilled water only ..	8	5	16—18 cm.	Slender, stiff and erect
b	.....	..	..	..	..
c	Calcium-free	7	5	30 cm.	Fairly stout. Larger leaves hanging
d	Ca-free + Fe	8	4	27—30 cm.	Stout. Larger leaves hanging
e	Magnesium-free	8	5	26 cm.	Fairly stout. Second leaves hang
f	Mg-free + Fe.....	7	5	28—30 cm.	Ditto.....
g	Nitrogen-free	8	3	30—32 „	Stout. Larger leaves hang
h	N-free + Fe.....	7	4	33 cm.	Fairly stout.....
i	Normal solution	6	3	26—28 cm.	Stout and well-developed
k	Normal + Fe.....	4	3	26 cm.	Ditto.....
l	Phosphorus-free	8	4	29 „	Slender and stiff....
m	P-free + Fe.....	8	4	28 „	Thin and stiff .....
n	Horse-dung decoction..	8	4	30—33 cm.	Stout. Larger leaves hanging
o	Horse-dung + Fe	7	6	27—28 „	Stout. Large leaves in part erect, in part hang over
p	Potassium-free	7	4	30 cm.	Stout, but larger leaves limp and hang
q	K-free + Fe	8	5	30 „	Fairly stout. Larger leaves hang

ne cleaned Sand to which Nutritive Salts, &c., were added, as before, in Beakers. Sow

Condition of plants.			Condition on August		
	Colour.	Average number of leaves.	Number showing infection.	Number infected on 1st leaf.	Number infected on 2nd leaf.
and	Rather pale, and first leaves withering; second leaves going at tips	3: maxm. breadth 3 mm.	0	0	0
	..	..	..	..	..
ger	Slightly pale. First, and some second leaves going at tips	4: maxm. breadth 6 mm.	5	3	5
ves	Slightly pale. First and second leaves touched at tips	$3\frac{1}{4}$: maxm. breadth 5—$5\frac{1}{2}$ mm.	4	1 and 1 flecked	4 and 2 flecked
ond	Rather pale. First leaves withered, second yellowing, and third have black tips	$3\frac{1}{4}$: maxm. breadth $4\frac{3}{4}$—5 mm.	2	0	1
...	Ditto.....	Ditto	1	0	1
ves	Rather pale, and first and second leaves black at tips	4: maxm. breadth 6 mm.	4	1	3
...	Good colour, but tips of first and some second leaves blackened	4: maxm. breadth $5\frac{1}{4}$ mm.	4	1	4
le-	Good colour, but first leaves begin to go at tips	3—$3\frac{1}{2}$: maxm. breadth 5 mm.	4	0	4
...	Good colour. First leaves just black at tips	3—$3\frac{1}{2}$: maxm. breadth 5 mm.	3	1	3
..	Pale. All leaf tips yellow ..	$2\frac{1}{2}$—3: maxm. breadth 2 mm.	?	0	3 flecked
..	Colour pale and first leaves going at tips, second yellow	$2\frac{1}{2}$—3: maxm. breadth 2 mm.	0	0	0
s	Colour good, but tips of first leaves begin to dry	$3\frac{1}{2}$—4: maxm. breadth $5\frac{1}{2}$ mm.	4	4	4
s	Good green colour throughout	$3\frac{1}{2}$—4: maxm. breadth 5—$5\frac{1}{2}$ mm.	6	4	6
	Colour good, but first leaves black at tips and second yellow	3—$3\frac{1}{2}$: maxm. breadth 5 mm.	4	1 and 1 flecked	4
	Colour good, but tips show traces of going	$3\frac{1}{2}$: maxm. breadth $5\frac{3}{4}$—6 mm.	5	4	5

. Sown July 10, and Infected on July 30 with Spores from *B. patulus*.

ber ted l leaf.	August 8. Number infected on 3rd leaf.	August 8. Degree of attack.	Condition on August 12.
)	0	0	Four second leaves show extremely minute pustules.
.	..	..	None of the seeds germinated.
5	1	Small as yet and young	Three first leaves and seven second leaves attacked, five of the latter *severely*. Deep green colour.
nd 2 ked	0	Ditto	Two first and four second leaves show very severe attack. Colour deep green, but pale around the infection areas.
1	? Flecked	Weak	Five weak and one severe injection area on second leaves, and two first leaves feebly touched. But all the second leaves are withering, and the whole pale.
1	0	Ditto	Four feeble and one severe infection area on second leaves. All pale, but better colour than *e*.
3	1 and ? flecked	Fairly strong attack	Three feeble areas on first leaves and four strong on second leaves. Pustules large and normal and leaves dark green.
4	1	Small	Two first leaves slightly infected, five second leaves with rather large pustules and good colour.
4	0	Ditto	Two feeble areas on first leaves, and four fairly strong on deep green second leaves.
3	0	Rather strong attack	Two feeble patches on first leaves, and three severe on deep green second leaves.
ecked	0	Flecks?	Four second leaves have extremely minute pustules. Colour pale.
0	0	0	One *extremely* minute pustule found on a second leaf. Colour pale.
4	1	Fairly strong attack	Four first leaves feebly, and four second leaves strongly attack. Good green colour.
6	2	Strongly developed patches	Four first leaves and six second leaves with strongly developed patches and pustules. Colour good green.
4	0	Fairly strong	Three first and six second leaves show pustules, the latter strongly. Colour good.
5	2	Small and young pustules	Five first and six second leaves show pustules, the latter rather strongly developed. Colour good.

Control cultures have shown over and over again that these seedlings do not incur the disease unless the spores are placed on the leaves, and I cannot accept these one or two apparent exceptions as evidence for a spontaneous outbreak of the disease.

The severity of the attack, as indicated by the size of the pustules and of the infected areas, and by the relative number of spores developed, followed much the same order as in the previous experiment, except that the nitrogen-free plants seemed to bear larger pustules than before.

The minute and poorly-developed pustules on the seedlings starved of phosphorus (*l* and *m*, Table III) or of all minerals (*a*) were particularly evident; as also were the severe attacks of the manured specimens. And, again, the magnesium-free seedlings showed premature yellowing and withering of the leaf-tips. Again, also, there was decided retardation of infection pustules in the phosphorus-free, distilled water, and, to a less extent the magnesium-free specimens.

But—again concordant—none of the plants were rendered immune from infection; and, as will be seen in the sequel, all the pustules, however minute, yielded spores with normal morphological characters, and perfectly capable of germinating and of reinfecting other seedlings of the *Bromus* in question.

Meanwhile, I had started a third complete series of sand-cultures of *Bromus pendulinus*, arranged as in the last series, but with two small variations in detail, viz., (1) in order to diminsh the risk of drowning or asphyxiating the seeds, I sunk a tall thistle-head funnel to the bottom of each beaker of sand, and poured the solution in each case down to the *bottom* of the beaker, allowing it to soak upwards and gradually moisten the upper layers from beneath; and (2) only 5 grains of the Brome (*B. pendulinus*) were sown in each beaker.

The results were very good. The grains were sown July 23. On July 29 seedlings were up in all but three of the beakers. On August 1 these also had seedlings showing, and by August 5 most of the seedlings were 25—30 mm. or more high.

On August 9 all but three of the beakers had four or five excellent seedlings, each with the second leaves showing, and the effects of the mineral starvation were beginning to be visible.

In order to emphasise this effect, if possible, by preventing the further development of the second and later leaves at the expense of materials stored in the *first* leaves, I now cut off the latter from each seedling with a pair of sharp fine scissors, and left the seedlings to grow further. During the next week the second leaf in each case attained its full size, and the third leaf began to appear; and I now proceeded to devote this series of seedlings to test the effect of sowing the spores reared on the seedlings of the last series (see Table III), and which had been affected as to their *numbers* by the starving or other

treatment of the seedlings, on the leaves of the seedlings of this series, similarly starved or otherwise.

As we have seen (Experiment series No. 47, Table I, and series 60, Table II), spores from a seedling starved of phosphorus, however few in *number*, are perfectly capable of infecting the leaf of a *normal* seedling. Can such spores also infect another seedling *similarly starved of phosphorus*; and can spores reared on calcium-starved or nitrogen-starved seedlings, &c., infect seedlings *similarly starved of calcium or of nitrogen* respectively, and so on?

On August 18, therefore, I infected the third leaf of each of several seedlings in each beaker of series 55 (Table IV) with the spores developed on the corresponding seedlings (which had been similarly treated) of series 50 (Table III).

The results are summarised in Table IV.

As the table shows, practically all the infections were successful, showing that not only does mineral starvation *not prevent the development of virulent spores* on the seedling so starved—if the latter is inoculated with normal spores—but such starvation is also *incapable of incapacitating the corresponding seedling for infection* by means of spores grown on *similarly starved seedlings*.

We must therefore conclude for the present that (1) the starvation of mineral food-substances, although it reduces the size of the host-plant and seriously diminishes the *quantity* of spores which the mycelium can give rise to on its leaves, does not affect either the *virulence* of such spores or the *predisposition to infection* of the leaves of the Brome concerned.

Moreover (2), in view of the results with the highly-manured seedlings to which horse-dung decoction or normal mineral solution was added, it seems hopeless to expect that high cultivation of this kind will *diminish the predisposition of the plant to infection*—or, what would amount to the same thing in practice—*increase its resistance or confer immunity*.

The effects of manurial treatment are clearly quantitative only, so far as this question is concerned.

If the host-plant is highly fed, its tissues yield more food materials for the fungus; the latter can develope a larger mycelium, and produce a larger crop of spores. But so long as the host-plant is capable of living at all, it is a perfectly satisfactory prey for the fungus in its tissues, so far as *quality* of fungus food is concerned.

It seems to me that these results throw some new light on the problem of infection and parasitism, in so far as they bear out the view that the Uredine mycelium taxes the leaf—robs it of a share of its food-supplies—rather than destroys the protoplasmic machinery, at any rate during the vigorous period of growth and of production of Uredospores; and also in so far as they suggest that whatever may be the

Table IV.—Exp. 55. Seedlings of *B. pendulinus* in Bea

No. 55.	Treatment.	No. of plates.	No. infected.	Source of infecting spores.	Germination of spores.	
a	Distilled water only.....	5	2	50, *a*	Good	Small, b
b	„ + Fe....	4	1	50, *a*	„	Very sm
c	Ca-free	4	4	50, *c*	Excellent ..	Much la
d	„ + Fe	5	5	50, *d*	Very good..	Similar,
e	Mg-free.....	5	4	50, *e*	Poor	Between
f	„ + Fe	5	5	50, *f*	Fair.....	Like *c*:
g	N-free.....	5	5	50, *g*	Very good..	Like *d*,
h	„	5	4	50, *h*	Fair.....	Like *b*,
i	Normal	4	4	50, *i*	„	Small in
k	„ + Fe	3	3	50, *k*	Poor.....	Small in pearin
l	K-free	5	4	50, *p*	Very good..	Fairly g
m	„ + Fe.....	5	3	50, *q*	Excellent ..	Small in
n	Horse	3	2	50, *n*	Good	Minute tions
o	„ + Fe	5	4	50, *o*	Poor	Ditto ..
p	P-free.....	5	3	50, *l*	Excellent ..	Very mi
q	„ + Fe	5	2	50, *m*	Very poor ..	Flecks o

pendulinus in Beakers of Fine Sand, sown July 23. Infected the 3rd Leaf on August 1

Germination of spores.	Condition on Augu 28.	Total plants.	Stature, &c.
Good	Small, but good infection	5	Small, with 2½—3 leaves.....
,, ...	Very small infection.....	4	Small, 3—3½ leaves.....
Excellent ..	Much larger infection: good spores....	4	Medium, 4—4½ broader leaves ....
Very good..	Similar, but younger infection	5	Medium, 4—5 leaves .....
Poor	Between *b* and *c* in degree of infection..	5	Large, 4—5 leaves.....
Fair.....	Like *c*: good infection	5	Large, 4—5 leaves.....
Very good..	Like *d*, but very small infection.....	4	Small, 4 leaves.....
Fair.....	Like *b*, very small infection	5	Small, 3½—4 leaves.....
,, ...	Small infection, but advanced.....	2	Medium, 4 leaves.....
Poor.....	Small infection: spores only just appearing	3	Fairly large, 4—4½ leaves.....
Very good..	Fairly good infection.....	5	Large, 4—4½ leaves.....
Excellent ..	Small infection.....	5	Large, 4—5 leaves.....
Good	Minute traces only of successful infections	3	Small, 3—3½ leaves.....
Poor	Ditto	5	Small, 3—3½ leaves.....
Excellent ..	Very minute traces of infection.....	5	Medium, 4 leaves.....
Very poor..	Flecks only visible	5	Medium, 3½—4½ leaves.....

ugust 18 with Spores of Exp. 50 (*B. pendulinus*).

Condition on September 1st.

	No. of plates with pustules.	Sizes, &c., of pustules and patches.
.....	3	Two very small patches, and two rather larger, with well developed pustules.
.....	1	Very small patch of very small pustules.
ces	4	Patches and pustules large and well developed.
.....	5	Patches large and pustules fine.
.....	4	Patches and pustules but little inferior to those of *d*.
.....	5	Patches and pustules large and well developed.
.....	4	Patches small, but pustules fine and large.
.....	4	Patches fair, and pustules large.
.....	1	Large and well-developed patches and pustules.
.....	2	Patches small, but pustules fine.
.....	4	Two patches large, two small: all pustules well developed.
.....	2	One patch small, one fairly large: all pustules well developed.
.....	2	Very small patches and minute pustules.
.....	4	Patches and pustules small, but well developed.
.....	3	Patches extremely small, with poorly developed minute pustules.
.....	2	Extremely minute and poorly developed patches and pustules.

causes at work in the living cell which confer immunity or predisposition on the species of host-plant, or which confer virulence or impotence on the spore, they lie deeper than nutrition, reminding us once more of the significant resemblances which, as I pointed out in a previous paper,* exist between the phenomena of infection and those of pollination. It is in the highest degree improbable that the pollen-tube of a given species, A, is incapable of growth in the style and the ovule of an allied species, B, simply because the tissues of B do not contain suitable food-materials, while the pollen-tube of a species, C, readily fertilises a more distant species, D, simply because the latter does contain suitable nutritive materials, especially as in both cases we may be able to germinate such pollen in artificial sugar-solutions.

All the evidence points to the existence, in the cells of the fungus, of enzymes or toxins, or both, and in the cells of the host-plant of anti-toxins or similar substances, as the decisive factors in infection or immunity, although I have as yet failed to isolate any such bodies.

Moreover, I regard the results here given as furnishing strong evidence, on the whole, against any hypothesis which assumes the existence of a latent or lurking source of disease in the plants themselves, and as supporting the view that every patch of pustules originates from a definite infection spot due to the entrance of a germ-tube from a spore which has there germinated on the leaf.

* 'Proc. Camb. Phil. Soc.,' vol. 11, part 5, p. 307.

"An Experimental Determination of the Variation of the Critical Velocity of Water with Temperature." By E. G. COKER, M.A. (Cantab.), D.Sc. (Edin.), Assistant Professor of Civil Engineering, and S. B. CLEMENT, B.Sc., Demonstrator of Civil Engineering, both of McGill University, Montreal. Communicated by Professor OSBORNE REYNOLDS, F.R.S. Received July 16,—Read November 20, 1902.

(Abstract.)

The change from stream-line to eddy motion in water was first examined by Osborne Reynolds, who in his earlier experiments, introduced colour-bands into a glass pipe in which water was flowing, to indicate the change in the motion, and later observed the resistance encountered in pipes over a great range of velocities.

The results of these experiments, and a consideration of the equations of motion, enabled him to express the laws relating to the critical velocity of water in pipes by the exceedingly simple equation

$$v_c = k \frac{\mu}{r\rho},$$

where v_c is the critical velocity of water, r is the radius of the pipe, μ is the viscosity of the water, ρ is the density, and k is some constant.

In the original experiments the range of temperature was very limited, and it was pointed out that "it would be desirable to make experiments at higher temperature, but there were great difficulties about this, which caused me, at all events for the time, to defer the attempt." It does not appear that such experiments have since been made, and although the difficulties were great, it was resolved to test the law through a much greater range than had hitherto been attempted.

Preliminary experiments showed that at temperatures beyond 50° C. the losses due to conduction and radiation were large, and that elaborate arrangements would be required to obtain reliable results. It was therefore decided to determine the variation of the critical velocity over a range extending from about 4° C. to 50° C., which seemed to be sufficient as a test of the law, and, with ordinary precautions, only necessitated small corrections for the effects of conduction and radiation. The resistance method used by Osborne Reynolds in his later experiments was employed, the arrangement only differing from his in details, such as the use of pressure chambers giving a continuous opening at the ends of the $\frac{3}{8}$-inch pipe examined, the employment of an inverted U-tube for measuring the pressures, and the weighing of the discharge.

Numerous experiments at different temperatures were made when stream-line motion was maintained in the pipe, and the relation of velocity to slope of pressure was determined by logarithmic plotting, giving a series of lines, the "logarithmic homologues" at different temperatures. The positions of these lines were found to be in substantial agreement with those calculated from the equations of motion. Similar experiments for eddy motion were made and the logarithmic homologues were also plotted, and their intersections with the corresponding ones for stream-line motion determined. These intersections give the minimum critical velocity, and were found to lie very approximately on a straight line in the diagram.

The law of variation of critical velocity, v_c, with temperature was found to be

$$v_c^{-1} \propto 1 + 0{\cdot}03368T + 0{\cdot}000156T^2,$$

where T is the temperature Centigrade, which agrees very closely with the known variation in the viscosity of water, viz.,

$$\mu^{-1} \propto 1 + 0{\cdot}03368T + 0{\cdot}000221T^2;$$

and it may, therefore, be concluded that over the range of temperature examined the critical velocity of water in small pipes varies directly as the viscosity.

"Isomeric Change in Benzene Derivatives—The Interchange of Halogen and Hydroxyl in Benzenediazonium Hydroxides." By K. J. P. Orton, Ph.D., M.A., St. John's College, Cambridge, Demonstrator of Chemistry, St. Bartholomew's Hospital. Communicated by Professor H. E. Armstrong, F.R.S. Received December 1,—Read December 4, 1902.

In discussing the laws which govern substitution in the case of benzenoid compounds, Armstrong, in 1887, drew special attention to the peculiar behaviour of amido- and hydroxy-compounds, from which he inferred that the phenomena of substitution were less simple than was commonly supposed. He showed that there was evidence that the formation of para-derivatives was preceded by that of an isomeric compound formed by the displacement of the aminic hydrogen or hydroxylic hydrogen, and pointed to the probability that this might prove to be true also of ortho-compounds.

Since that time, it has been experimentally demonstrated by various chemists that the radicles, Cl, Br, I, NO_2, SO_3H, can all be introduced in place of the hydrogen of the amino-group of anilines and of the imino-group of anilides, and that the compounds thus formed can be

changed into isomeric substances in which the substituent is contained in the hydrocarbon nucleus.

The transformation of the aniline derivative appears to take place only in the presence of some other substance: in the case of the phenylacylchloramines, such as $C_6H_5.NCl.COCH_3$, for example, apparently the change into the isomeric chloroacetanilides takes place only under the specific influence of hydrogen chloride.* The investigation of such cases of isomeric change is, in fact, of special interest as they are, so to speak, "fermentative" in character, often taking place with remarkable facility, and under the influence of minute amounts of the substance, which apparently provokes the change—the catalyst. Measurements of the velocity at which changes of this type occur, for example, of diazoaminobenzene into aminoazobenzene,† show that a velocity-coefficient of constant value is given by the equation,

$$k = t^{-1} \log a/(a-x).$$

The isomeric change is, therefore, apparently a so-called monomolecular change; but, as in other cases, the slowest of the series of simple changes which make up the complete transformation is alone measured; moreover, the substance which conditions the change is not taken into account.

This paper deals with a new case of intramolecular change of a particularly interesting character: that of *s*-trichloro- and *s*-tribromobenzenediazonium hydroxide, $C_6H_2X_3.N(OH):N$, into hydroxybenzene derivatives, by the interchange of the hydroxyl for one of the ortho-halogen atoms. The change takes place under all conditions under which it is possible for the diazonium hydroxide to be present. Thus in dilute aqueous solutions of the neutral diazonium nitrate, or even of the hydrogen sulphate, chloride is just recognisable by means of silver nitrate after 24 hours; at the same time the solution becomes yellow, owing to the formation of the diazophenol. But as the extent to which the nitrate, and more especially the sulphate, undergoes hydrolytic dissociation is extremely small, the isomeric change takes place very slowly; and in the presence of a considerable excess of acid no appreciable change occurs during 5 days. On the other hand, using diazonium acetate, 50 per cent. of the material changes within 30—40 hours; and in the case of diazonium bicarbonate the change is nearly instantaneous. That the change is a transformation of the diazonium hydroxide is further emphasised by the fact that the addition of a solution of the diazonium salt to a considerable excess of an aqueous solution of an alkali carbonate, is not followed by elimination of halogen; the solution remains colour

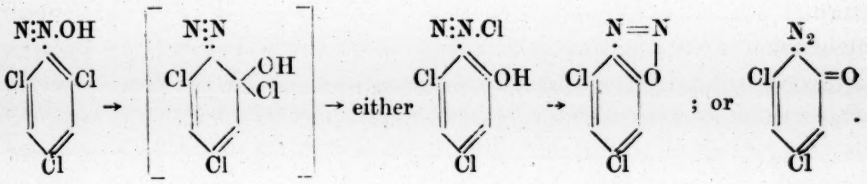
* Compare Armstrong, 'British Association Report,' 1899, p. 683; 'Trans. Chem. Soc.,' 1900, vol. 77, p. 1053.

† Goldschmidt and Reinders, 'Berichte,' 1896, vol. 29, p. 1369.

less, the diazonium salt being immediately converted into the alkali diazotate.* The latter change may be represented thus—

$$\underset{\text{Diazonium salt.}}{C_6H_2X_3.NR{:}N} \rightarrow \underset{\text{Diazonium hydroxide.}}{C_6H_2X_3.N(OH){:}N} \rightarrow \underset{\text{Diazotate.}}{C_6H_2X_3.N{:}N{\cdot}OM'}.$$

As the author has maintained on previous occasions, the first step in this type of intramolecular change consists in a transference of the atom or group attached to the nitrogen to the ortho- or para-carbon atom of the nucleus, an ortho- or para-quinone being formed. The complete transformation in the case here described may be represented in the following manner :—



I. II. III. IV. V.

In other words, the diazonium chloride (III) may be represented as changing into the "diazophenol" (IV); or, if, following Wolff† and Hantzsch,‡ the diazophenols are considered to be diazoquinones (V), the ortho-quinone form (II) merely loses hydrogen chloride.

Closely allied to the intramolecular change just mentioned, are Hantzsch's observations§ that *s*-tribromobenzenediazonium chloride changes into chlorodibromobenzenediazonium bromide, and chloro- and bromo-benzenediazonium thiocyanates into thiocyanobenzenediazonium chlorides and bromides.

[The following observations of Meldola‖ may also be mentioned, as the author's results offer a possible explanation. When dinitro-*o*- or dinitro-*p*-anisidine is treated with sodium nitrite in the presence of acetic acid, the nitro-group occupying a position ortho or para with respect to the amino-group is eliminated, a diazophenol (diazo-

* Professor Meldola has called the author's attention to a French patent (No. 315,932) of the Badische Aniline Company, dated February 28, 1902, in which the replacement of the nitro-group and halogen by the hydroxyl-group *by the action of alkalis* on diazonium salts, is claimed as a technical process. The author's experiments show within what limits this process is applicable, and what is the probable action of the alkali.

† 'Annalen,' 1900, vol. 312, p. 119.

‡ 'Berichte,' 1902, vol. 35, p. 888.

§ 'Berichte,' 1896, vol. 29, p. 947; 1897, vol. 30, p. 2334; 1898, vol. 31, p. 1253; 1900, vol. 33, p. 505.

‖ 'Trans. Chem. Soc.,' 1900, vol. 77, p. 1172; 'Proc. Chem. Soc.,' 1901, vol. 17, p. 131; 'Trans. Chem. Soc.,' 1901, vol. 79, p. 1076; 1902, vol. 81, p. 988.

oxide) being formed. In diazotising in the presence of hydrogen chloride, an ortho-nitro group is replaced by chlorine.]

The isomeric changes of the diazonium salts differ, however, somewhat from the similar changes of the phenylacylchloramines, phenylnitramines, &c. The latter compounds are *per se* relatively stable, and apparently only undergo change in the presence of some agent (catalyst); the diazonium compounds, on the other hand, appear to be intrinsically labile, and in aqueous solution, at least, capable of passing into a more stable configuration. In the latter case the nitrogen atom, bearing the wandering group, the hydroxyl group (or, as in Hantzsch's instances, the chlorine atom or the thiocyanate group), is pentad, thus, Ph.N(OH):N, whilst in the phenylacylchloramines, &c., it is triad. The idea at once suggests itself that in the case of the last-mentioned compounds, the first action of the catylist is to form an additive product, in which the nitrogen is pentad. The product, in which, it should be noted, more than one negative radicle is attached to the nitrogen, is not a stable compound, and is now capable of passing into the isomeric quinone form, and thus start the transformation.

These results are also of interest, inasmuch as Hantzsch* has stated recently that he has obtained *s*-tribromophenylnitrosamine, $C_6H_2Br_3.NH.NO$, by adding sodium acetate to a solution of a *s*-tribromobenzenediazonium salt. He describes it as a bright yellow amorphous substance, which decomposes at 85°. As far as can be judged, it is this very reaction which has been studied in the course of the author's experiments. The substance which is precipitated is at first glance a yellow amorphous powder, but close observation shows that long ($\frac{1}{2}$—1 cm.) orange crystals are present. These crystals are the 3 : 5-dibromo-*o*-diazophenol hereafter described. The powder is probably a hydroxyazo-condensation product. Hantzsch does not appear to have observed that bromine is eliminated. He also affirms (*loc. cit.*) that he obtained the nitrosamine by the cautious addition of acetic (or other) acid to the alkali diazotate; in the writer's experience, however, this always leads to the elimination of halogen.

The Transformation of s-*Trichlorobenzenediazonium Hydroxide.*

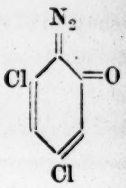
s-*Trichlorobenzenediazonium hydrogen sulphate*, $C_6H_2Cl_3.N(SO_4H):N$, is very easily prepared by diazotising, by means of amyl nitrite, *s*-trichloraniline dissolved (or suspended) in glacial acetic acid containing sulphuric acid. The salt is precipitated by ether, and is purified by dissolving in methyl alcohol; on adding ether to this solution the sulphate separates in small colourless lustrous prisms, often forming

* 'Berichte,' 1902, vol. 35, p. 2964.

star-shaped aggregates, which are very soluble in water (SO_4 found 31·0, calculated 31·4 per cent.). *s-Trichlorobenzenediazonium nitrate*, $C_6H_2Cl_3{\cdot}N\,(NO_3){:}N$, prepared in a similar manner, forms colourless needles. When kept for two or three days, both salts begin to show signs of change, which are more marked in the case of the nitrate. The latter becomes noticeably yellow, and when dissolved in water forms a yellow solution. The initially colourless aqueous solution of the pure colourless salts becomes yellow after a few hours; after 24 hours chloride is just recognisable in the solution. In one experiment, 0·5 gramme of the acid sulphate was dissolved in 100 c.c. of water; after 3 days a very small amount of a yellowish-red solid had separated from the yellow solution; at the end of 16 days, the chloride in solution was precipitated by silver nitrate in the presence of nitric acid: the silver chloride weighed 0·05 gramme, whereas for the complete conversion of the diazonium salt into the diazophenol, 0·235 gramme of silver chloride should have been found.

An aqueous solution of *s*-trichlorobenzenediazonium acetate, obtained by mixing neutral solutions of the diazonium nitrate and sodium acetate (molecular proportions) rapidly becomes yellow and acid. In a short time the solution becomes turbid, and after 4—5 hours deposits a bright yellow amorphous solid. In an experiment, in which 1·75 gramme of the diazonium nitrate, dissolved in 150 c.c. of water, was treated with sodium acetate (1 mol.), and kept at 10—15° for 40 hours in the dark, an estimation of the hydrogen chloride in the solution showed that 54·5 per cent. of the diazonium compound had changed into the diazophenol.

When instead of sodium acetate, sodium hydrogen carbonate was used a similar change took place, but far more rapidly. In one experiment a dilute aqueous solution of sodium hydrogen carbonate (3 mol.) was added drop by drop during a period of 1 hour to a cooled solution of 1 gramme of the diazonium hydrogen sulphate. (The bicarbonate was finally present in sufficient quantity to combine with the whole of the sulphuric acid and one-third of the chlorine present in the diazonium salt.) After one-third of the bicarbonate had been added, and the acid converted into the normal sulphate, the solution rapidly became yellow and deposited a yellow solid. Throughout the experiment the mixture was acid. As soon as the whole of the bicarbonate was added, the chlorine was estimated; it represented 54·5 per cent. of the amount which should be obtained were 1 atom of chlorine eliminated from the diazonium salt. In another experiment, using the same quantities of diazonium salt and sodium bicarbonate, the solutions were mixed as rapidly as possible; a copious yellow precipitate at once appeared; the chloride in the filtrate represented 72 per cent. of 1 atomic proportion of chlorine.

3 : 5-*Dichloro*-o-*diozophenol* (3 : 5-*dichloro*-o-*diazoquinone*),  , is

contained in the yellow solutions which are obtained by any of the methods just described. It is best prepared by adding excess of sodium acetate to *s*-trichlorobenzenediazonium hydrogen sulphate or nitrate. The mixture should be kept during 40—50 hours in the dark;—in the light the solution darkens, the diazophenol decomposing. The liquid was then filtered from the amorphous yellow solid, made strongly acid with nitric acid, and extracted four or five times with ether. On evaporating the yellowish-brown extract, a mixture of oil and crystals remained. In order to obtain the diazophenol, dry hydrogen chloride was passed into the ethereal extract; this caused the *hydrochloride* of the dichloro-*o*-diazophenol, $O{:}C_6H_2Cl_2{:}N_2,HCl$, to separate in small needles. This salt was converted into the diazophenol by treatment with a small quantity of water. Thus prepared, the diazophenol is an orange powder, which crystallises from an ethereal solution in flattened orange prisms, which melt at 83—84°, forming a red liquid; at 87° the latter decomposes. On analysis, 0·1068 gramme gave 0·1618 gramme AgCl. Cl = 37·45.

0·2004 gramme gave 25·4 c.c. of moist nitrogen at 14° and 766 mm. N = 14·95. $C_6H_2ON_2Cl_2$ requires Cl = 37·57; and N = 14·86 per cent.

This substance is very readily soluble in all solvents except petroleum; when dissolved in hot water it rapidly decomposes, the yellow solution becoming brown and turbid. It dissolves in concentrated solutions of acids, forming a nearly colourless liquid, which becomes yellow on adding water. These acid solutions couple with alkaline solutions of β-naphthol. There is no doubt that this substance is an ortho- and not a para-diazophenol, as *p*-diazophenols can be easily recrystallised from hot water, whilst the ortho-compounds are decomposed by hot water. Again, *p*-diazophenols are easily reduced to *p*-aminophenols, but the ortho-derivatives are not reduced in a simple manner.*

The amorphous yellow substances which are formed in the cases above described appear to be condensation products. The *o*-diazophenol which is produced has the para- and one ortho-position unoccupied, and being formed in the presence of a diazonium salt and sodium acetate or bicarbonate, is under conditions which are very favourable to the production of hydroxyazo-derivatives. A similar yellow powder is precipitated when sodium bicarbonate is added to a solution of the 3 : 5-dichloro-*o*-diazophenol. The yellow substances

* Böhmer, 'Journ. Prakt. Chem.,' 1881, [2], vol. 24, p. 460.

cannot be got to crystallise from any solvent; they decompose at 85°, and in a moist atmosphere slowly give off nitrogen; this is very noticeable when an attempt is made to estimate the nitrogen by Dumas' method. They do not dissolve in aqueous alkalis or acids. Cryoscopic determinations of the molecular weight in benzene show that, whether prepared by means of sodium acetate or bicarbonate, the molecule contains three benzeneazo-groups. There are three such compounds possible, formed by condensation either of (1) 2 mols. of *s*-trichlorobenzenediazonium salt with 1 mol. of the diazophenol $((C_6H_2Cl_3\cdot N_2)_2\cdot C_6Cl_2\ll^{N_2}_{O}$ has a mol. weight = 604; chloride = 46·8 per cent.); or (2) of 1 mol. of the diazonium salt with 2 mols. of the diazophenol (mol. weight = 585·5; chlorine = 42·2 per cent.); or (3) of 3 mols. of the diazophenol (mol. weight = 567; chlorine = 37·5 per cent.). The material prepared by the use of sodium acetate has the highest percentage of chlorine, 44·7—44·9; its molecular weight was found to be 580—590; it is probably a mixture of (1) and (2). When bicarbonate is used, the yellow substance contains 41·4—43·7 per cent. of chlorine; its molecular weight was found to be 560—580.

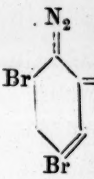
The Transformation of s-*Tribromobenzenediazonium Hydroxide.*

The transformation of *s*-tribromobenzenediazonium hydroxide resembles in its main features that of the *s*-trichloro-compound. *s*-Tribromobenzenediazonium nitrate, when first prepared, is pure white, although Silberstein* has described it as yellow. On keeping, it gradually acquires a yellow colour. Solutions both of the nitrate and the hydrogen sulphate become yellow, and bromine can be detected in the liquid. When either sodium hydrogen carbonate or sodium acetate is added to solutions of the salts, the decomposition is far more rapid. Thus in one experiment, in which sodium bicarbonate was added to a solution containing 1 gramme of the diazonium hydrogen sulphate, 0·31 gramme of silver bromide was obtained, whereas 0·508 gramme is required for the elimination of one atom of bromine.

In the presence of sodium acetate, the solutions of the diazonium salt rapidly become yellow and turbid. After 24 hours at 0°, a bulky and, at first glance, homogeneous precipitate had separated. In one experiment when 2 grammes of the sulphate were used, 0·58 gramme of silver bromide was obtained from the filtrate from the yellow precipitate. Careful examination of the yellow precipitate showed that it consisted of crystals completely covered and matted together by an amorphous yellow powder. These two substances could not be separated by crystallisation from any solvent. It was, however, found possible

* 'Journ. Prakt. Chem.,' 1883, [2], vol. 27, p. 113.

to obtain the crystals free from the powder by frequently shaking up the solid with water and decanting the supernatant liquor before the light powder had had time to settle.

3 : 5-*Dibromo*-o-*diazophenol* (3 : 5-*dibromo*-o-*diazoquinone*), 

The crystals, just mentioned, are the 3 : 5-dibromo-*o*-diazophenol in a state of purity. They are long slender transparent prisms of a fine orange colour, which explode when heated at 140°.

0·1894 gramme gave 0·2564 gramme AgBr. Br = 57·6.

Two cryoscopic determinations of the molecular weight in benzene solution gave the values 278·9 and 281·6.

$C_6H_2ON_2Br_2$ requires Br = 57·53 per cent., and a molecular weight of 278.

The compound is very soluble in chloroform, benzene, ether and glacial acetic acid, and in boiling alcohol. It is moderately soluble in boiling water, and very slightly so in cold water or petroleum. Treatment with hot water or alcohol decomposes it. It dissolves in concentrated acids, forming a colourless solution, and is reprecipitated by addition of water. The solution in acids couples with alkaline solutions of β-naphthol. The hydrochloride is obtained in nearly colourless needles by passing dry hydrogen chloride into a dry ethereal solution. This substance is without doubt the ortho-diazophenol, and not the 3 : 5-dibromo-*p*-diazophenol, which has been prepared by Silberstein (*loc. cit.*), and which can be crystallised from hot water.

In the account given by Hantzsch, of the experiments above referred to, it is stated that on adding sodium acetate to the solution of a *s*-tribromobenzenediazonium salt, the whole of the latter is gradually converted into *s*-tribromophenylnitrosamine, $C_6H_2Br_3.NH.NO$, of which analyses are given: it forms an amorphous yellow powder, decomposing at 85°. By passing hydrogen chloride into the ethereal solution, he obtained a hydrochloride of the nitrosamine, which is immediately decomposed by water, a property possessed by the hydrochloride of the diazophenol.

The amorphous yellow powder, obtained by the author, both by the action of sodium hydrogen carbonate on the *s*-tribromobenzenediazonium salts, and mixed with crystals of the diazophenol when sodium acetate is used, decomposes at 85—90° (compare Hantzsch, 85°). The yellow substance appears to be similar to those obtained from *s*-trichlorobenzenediazonium salts; but determinations of the molecular weight, indicate that only two benzeneazo-groups have condensed. In this case, however, the yellow powder certainly contained some of the diazophenol, as on passing hydrogen chloride into an ethereal solution

of the powder, a small quantity of the hydrochloride of the last-mentioned substance separated.

[The investigation of these intramolecular changes is being continued. It has been found that a similar interchange of halogen for hydroxyl takes places very readily in solutions of chloro- and bromo-naphthalene-diazonium salts, even in the presence of excess of acid. The author is of opinion that the observation of Gaess and Ammelburg,* that an aqueous solution of 1-nitro-2-naphthalenediazonium sulphate yields the 1 : 2-naphthalenediazo-oxide, is an example of the type of transformation here considered.]

"On certain Properties of the Alloys of the Gold-Silver Series." By the late Sir W. C. Roberts-Austen, K.C.B., D.C.L., F.R.S., and T. Kirke Rose, D.Sc. Received October 22,—Read December 11, 1902.

[Plate 3.]

In a former communication to the Society† the curve of the initial freezing points of the alloys of gold and copper and some micrographic evidence as to their structure were given, and it was shown that according to the theory of solutions the alloys rich in gold should not be homogeneous after they have solidified. The fact that they are not uniform was confirmed by analysis. The subject has, however, more than theoretical interest, and the inference was drawn that standard gold, which consists of eleven parts by weight of gold to one of copper, is unsuitable as a material for the preparation of the trial plates by which the standard of the coinage is tested. These trial plates according to law must contain 916·6̇ parts of gold and 83·3̇ of "alloy," that is of some other metal, and it remained to be determined what the other metal should be.

It will be at once apparent that the alloy or mixture of the two metals must, if the cold mass is to be uniform, solidify as a whole, that is to say, that the crystals first formed should be of the same composition as the mother liquor, and this condition can be fulfilled by isomorphous mixtures only. It has long been recognised that the gold-silver alloys are cases of isomorphism, and Gautier, in 1896, stated‡ that the freezing-point curve of the series followed a straight line if the percentages by weight of the constituents were taken as abscissæ.

This curve was re-determined by experiment, a number of alloys being made up and autographic records taken of their cooling curves by the Roberts-Austen recording pyrometer. The results obtained

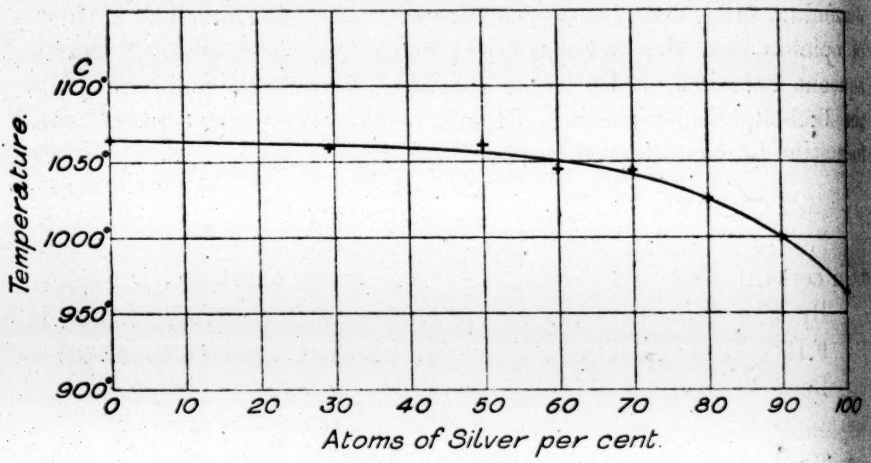
* 'Berichte,' 1894, vol. 27, p. 2211.

† 'Roy. Soc. Proc.,' vol. 67 (1900), p. 105.

‡ 'Bull. de la Soc. d'Encouragement,' Oct., 1896.

are given in the following table, and have been plotted in fig. 1, in which the abscissæ are atomic proportions of the metals in the alloys. The freezing point of gold was taken as 1064°.

Fig. 1.



Percentage of gold present in alloy.		
By weight.	In atoms.	Freezing point.
100	100	1064°
80·99	70·25	1061
64·60	49·97	1061
54·80	39·89	1046
43·98	30·07	1044
31·71	20·28	1028
17·23	10·23	1001

The following points had been observed by Heycock and Neville*:—

2·26	1·25	962°
0·91	0·50	961
0	0	960

It will be seen that Gautier's conclusion is substantially confirmed, but it was observed, as one of us had previously pointed out,† that the first additions of silver did not depress the freezing point of gold. So far does this property extend that even the alloy containing 50 atoms of gold to 50 of silver, or 64·6 per cent. of gold by weight, solidifies at 1061°, which is only 3° below the freezing point of pure gold. With further additions of silver there is a steady acceleration in the rate of lowering of the point of solidification, so that the freezing-point curve of the series has no double flexure, unless one is indicated near the silver end of the curve by Heycock and Neville's results.

* 'Phil. Trans.,' A, vol. 189 (1897), p. 69.

† Roberts-Austen, 'Proc. Inst. Mechanical Engineers,' 1891, p. 564.

FIG. 2.

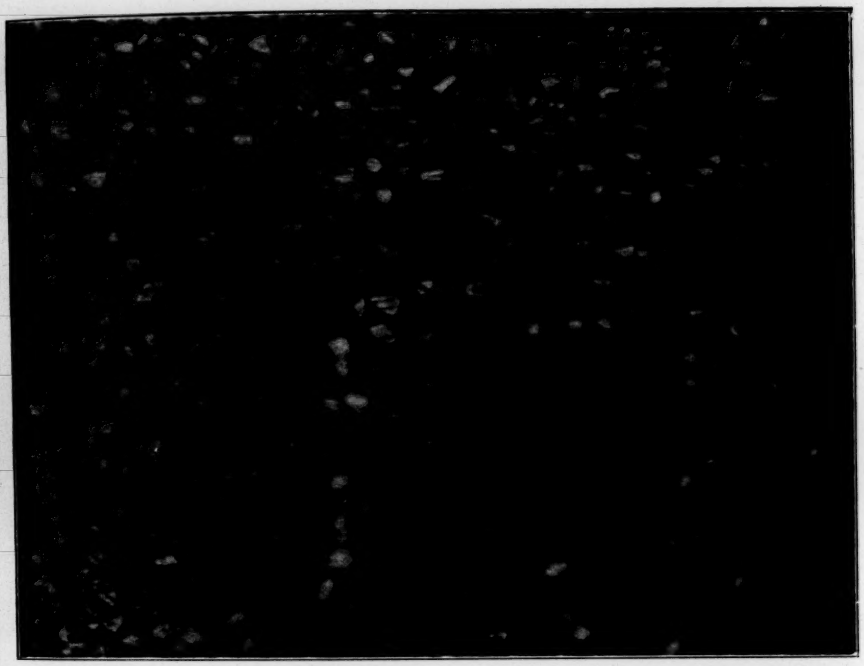


FIG. 3.



There is, of course, no eutectic alloy observable in any member of the series.

The alloys all consist of large grains, but these are built up of smaller grains, so that the ultimate structure is exceedingly minute. When magnified 1500 diameters the grains appear as small irregular crystals of the cubic system (see fig. 2, Pl. 3). In order to develop any segregation that might take place, an ingot of the standard alloy containing 91·66 per cent. of gold by weight was heated for 2 months in one of the annealing furnaces at the Royal Mint, the temperature of which was kept at about 700° by day, but fell to about 100° at night. The maximum temperature attained was over 300° below the fusing point of the alloy, and the sharpness of the angles of the specimen had suffered no change. After this treatment it was found that the grains had increased in size, and the crystals forming them had become well developed, as shown in fig. 3 (Pl. 3), in which the structure is magnified 1500 diameters. No true segregation, however, could be detected even in this ingot, either by analysis or by the microscope, and plates prepared by rolling out ingots containing 916·6̇ parts by weight of gold, and 83·3̇ parts of silver, were found on analysis to be uniform in composition.

The ancient trial plates, according to the analysis made by one of us,* consist of a triple alloy of gold, silver, and copper. The earliest one in existence was made in 1527, the year following the first introduction of the standard 916·6̇. This plate contained only 0·62 per cent. of copper, and was probably intended to consist of gold and silver only. All subsequent plates, however, down to that made in 1829, contained much larger amounts of copper. In 1873 it was determined to omit the silver and to use only copper as the alloying metal, and thus to preserve identity of composition between the trial plate made in that year and the coinage. In view, however, of the importance of obtaining homogeneous trial plates and of the ease with which the exact quantity of copper required to make the assay pieces identical in composition can be added to the pieces of the trial plate during the course of the assays, it is preferable to use only silver as the alloying metal in the manufacture of the trial plates.

Such an alloy has accordingly been used at the Royal Mint since the beginning of the present year instead of fine gold for checks in the assay of standard bars and coins. In view of the minute accuracy with which the operations of coinage have to be conducted, this is a matter of much importance. By this method any errors are avoided which might be caused by accidental variations in weights occurring after the trial plates have been made.

* Roberts-Austen, 'Chem. Soc. Journ.,' 1874, p. 197.

"Abnormal Changes in some Lines in the Spectrum of Lithium."

By HUGH RAMAGE, B.A., St. John's College, Cambridge. Communicated by Professor G. D. LIVEING, F.R.S. Received November 19,—Read December 11, 1902.

In the course of an investigation on the flame spectra of metals, the writer has examined the spectrum of lithium. Some facts have been discovered of sufficient importance for a separate paper.

The same spectrometer was used as in the investigation on the "Spectra of Potassium, &c."* Whilst the wave-lengths of some of the lines in the flame spectrum of lithium agree closely with those given by Kayser and Runge for the lines in the arc spectrum,† the wave-lengths of other lines differ considerably from these. The numbers and differences are given in the following table :—

Oxyhydrogen flame spectrum (Author).		Arc spectrum (Kayser and Runge).					
		Principal series.		Second subordinate series.		First subordinate series.	
Wave-length.	Intensity.	Wave-length.	Difference from flame.	Wave-length.	Difference from flame.	Wave-length.	Difference from flame.
6708	10	6708·2	—				
6103·84	9					6103·77	−0·07
4971·98	2			4972·11	+0·13		
4603·07	7					4602·37	−0·70
4273·34	1			4273·44	+0·10		
4132·93	5					4132·44	−0·49
3985·86	<1			3985·94	+0·08		
3915·59	3					3915·2	−0·39
3795·18	2					3794·9	−0·28
3719	1					3718·9	—
3232·82	4	3232·77	−0·05				
2741·43	1	2741·39	−0·04				

The flame lines are all sharp.

Kayser and Runge describe the lines 6708·2, 3232·77, 2741·39, and 6103·77 as "mostly reversed"; the lines in the second subordinate series as diffuse towards the red; the lines 4602·37 and 4132·44 as reversed; and the latter, with the remaining lines in the same, series as diffuse on both sides.

* 'Roy. Soc. Proc.,' vol. 70 (1902), p. 303.

† 'Berlin Akad. Abhandl.' (1890), vol. 4, p. 19.

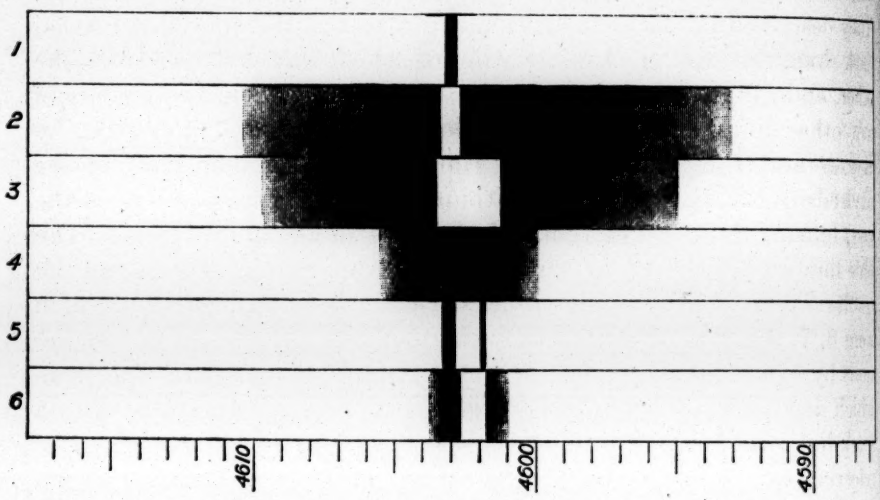
The only important differences occur in the lines of the first subordinate series. Exner and Haschek have given the wave-lengths of three lines in the spark spectrum* as 2815·55, 3232·91, and 4603·10. The last is described as reversed. Eder and Valenta have given the wave-length of the blue line in the Bunsen flame spectrum as 4602·4,† and as 4602·46 in the spark spectrum when a condenser was used.‡ They described the latter measurement as that of the middle of a broad dark line, the less refrangible wing of which was stronger than the other, and diffuse towards the red. They also gave measurements of five other lines, and these agree closely with the arc lines given by Kayser and Runge. Professor Hartley recorded four lines in the oxyhydrogen flame spectrum of lithium chloride§ corresponding to the first, fourth, sixth and eleventh of the lines in the above table. The blue line was measured in the flame spectrum on four plates, and the results differed only in the second decimal place, the figures in which were 8, 7, 6 and 7. Professors Liveing and Dewar made some observations by eye on the appearance of the blue line in the arc spectrum which led them to believe there were two lines, a strong one with a weak line on the more refrangible side,‖ and Kayser and Runge, after referring to this, say :—¶ " Wir haben nur bei zwei Aufnahmen neben der Hauptlinie eine zweite schwache umgekehrte Linie bei 4603·13 erhalten ; da aber hier eine Eisenlinie liegt, glauben wir, dass dies eben die Eisenlinie ist, welche sich durch den hellen Hintergrund der Lithiumlinie umgekehrt hat."

The wave-length of the line which Kayser and Runge attributed to iron agrees with that obtained by the author for the bright line in the flame spectrum, and by Exner and Haschek for the reversed line in a spark spectrum. It was decided in view of these differences to take a series of photographs of the arc spectrum of lithium, using the carbonate of lithium on carbon poles, and to study especially the appearances of the blue line. The results will now be briefly described.

A Gulcher arc lamp with vertical carbons was used. When the arc was started with a quantity of lithium carbonate on the carbons, a large proportion of the salt was freely volatilised and expelled in the form of a dense vapour. As the arc lamp was placed, the magnetic field produced by the feed mechanism caused the bulk of the vapour to be expelled in the direction of the collimator. Photographs of the spectrum taken at this early stage show the line as a very broad bright line extending from 4610·4 to 4593·5, with a narrow dark line extend-

* 'Sitzber. kais. Akad. Wien,' vol. 106, Abth. 2A (1897), p. 1133.
† 'Denkschr. kais. Akad. Wien,' vol. 60 (1893).
‡ 'Denkschr. kais. Akad. Wien,' vol. 67 (1898).
§ 'Phil. Trans.,' A, vol. 185 (1894), p. 177.
‖ 'Phil. Trans.,' vol. 174 (1883), p. 215.
¶ *Ibid.*, p. 20.

ing (2 in figure) from 4603·46 to 4602·71, having its middle (measured) at 4603·06. The middle of the broad bright line is, according to the above, at wave-length 4601·95, and the middle of the dark line (reversed) is coincident with the bright line of the flame spectrum.



Diagrams of the blue lithium line as it appears in photographs. The fine vertical lines are merely shading lines; the broad wings of the actual lines in the spectra are continuous.

(1) Bright line of flame, arc and spark spectra.
(2) Reversed line from dense outer flame of arc.
(3) Reversed line from inner cone of strong arcs and from near negative pole of weak arcs.
(4) Broad line with two absorption bands, from arc spectrum.
(5 and 6) From spectrum of thin outer flame of arc with different exposures.

One pole of a weak bar magnet was placed near the arc so as to direct the expelled vapour in a direction perpendicular to the axis of the collimator. After the large excess of the lithium salt had been volatilised the image of the central portion of the arc was thrown on the slit, and the spectrum photographed, when the ends of the carbon poles, not widely separated, were red-hot. The lithium vapour in the central core was doubtless at a very high temperature, and there was only a thin layer of cooler vapour between it and the slit. Under these conditions the bright line is again broadly expanded, and so also is the dark line, for it now extends from 4603·55 to 4601·25 (mean = 4602·40) on one photograph (3 in figure), and from 4603·68 to 4601·22 (mean = 4602·45) on another. The bright wings are nearly of the same intensity and extent. The middles of these two dark lines are nearly the same as the wave-length 4602·37, given by Kayser and Runge, and by Eder and Valenta for the reversed lines.

Some photographs were also taken of the spectrum of the transparent flame which issued from the arc, the bar magnet being used as above. With a short exposure one narrow bright line was obtained of wave-length 4603·06; with a longer exposure this bright line was much stronger, and there was the appearance of a weak line on its more refrangible side, exactly as Professors Liveing and Dewar described. The wave-lengths of these two were 4603·18 and 4601·89 respectively on one photograph (5 in figure), and 4603·12 and 4601·65 on another. The last line was fairly sharp on the less refrangible side, whilst it faded gradually on the other side (6 in figure). With still longer exposures the weaker line could not be distinguished from the wing of the much expanded bright line. This wing when weak always resembles a line, and it was thought that a line might be nearly coincident with its less refrangible edge. Measurements showed that this edge varied in position on different photographs, but Professor Liveing suggested that the tip might be formed by a line, and if so, the tip should always have the same wave-length. The following table gives the wave-lengths of the two tips on different photographs:—

Spectrum.	Tip of weaker side.	Tip of stronger side.
206^3	4601·68	
213^1	·83	4603·11
213^6	·66	·11
214^1	·72	·03
222^6	·78	·10
222^7	·82	·11
224^6	·82	
226^5	·83	
226^7	·95	4603·21
232^6	·79	

This evidence is not conclusive; both tips appear to vary, but the weaker one varies more than the other. The arc, in these experiments, was formed with a Gulcher lamp with the carbons vertical, the positive being uppermost, and the image of the arc projected on to the slit of the collimator by a lens. It was observed that the less refrangible point, wave-length 4603·1, was, in many photographs, higher on the plate than the weaker point; it was given out by vapour quite near to the positive pole. It was observed also in some photographs that the line extending from it faded away lower down; the vapour near the negative pole did not emit this line but gave the broad bright line with the broad dark line down its middle. This broad bright line fades away as the positive pole is approached; the more refrangible wing ends in a point, but the less

refrangible wing is lost in the bright line of wave-length 4603·1. There appears then in the middle portion of the spectrum a broad dark line with wings of unequal extent, and the less refrangible wing is broader, and, near its more refrangible edge, much stronger than the other wing. The broad dark line extended on such photographs from (1) 4602·72 to 4601·81, (2) 4602·86 to 4602·05, (3) 4602·73 to 4601·62, and in two other cases, where there were less differences of intensity between the two wings, from 4603·26 to 4601·61 and from 4603·07 to 4601·59. There were indications on some photographs of a bright line near the middle of the broad dark line; the measurements of such a photograph gave :—

Less refrangible edge of broad dark line	4603·40
Apparent bright line	4602·57
More refrangible edge of broad dark line.....	4601·31

The absorption bands of reversals in such a photograph as this are comparatively bright, for there is considerable action on the plate where the images of these bands fall (4 in figure). The effect is probably due tothe superposition of the spectrum of the outer flame upon that of the inner core when both are giving reversed lines.

Some observations were made at Professor Liveing's suggestion on the arc spectrum of lithium when the arc was formed between carbon electrodes inserted horizontally in a magnesia brick. The arc enclosed in this way is much steadier than in the open. The light which was examined passed out through a horizontal hole perpendicular to the carbons. The differences described above between the spectra of the vapours near the two poles were easily observed by eye observations.

Photographs of the spark spectra of lithium were taken in the hope of finding a second line which would account for the remarkable differences in the arc spectra, but no second line was found. A piece of metallic lithium was placed in a cup formed in the end of an aluminium rod, and sparks were passed between this and an aluminium wire held immediately over the lithium. No Leyden-jar was put into the secondary circuit at first, and it was found that photographs of the blue line were obtained with exposures of 15 minutes. The lines were sharp, and showed no signs of reversal. The wave-length of the blue line was found to be 4603·08. The vapour near the electrodes gave broader lines than the vapour a short distance away. The broadening was much greater at the negative electrode than the positive, and it extended further on the more refrangible side than on the other side. The bright line and the broad reversed line were, in fact, both present in the spectrum of the spark, near the negative electrode.

It was much more difficult to obtain photographs of the spark spectrum when a Leyden jar was introduced. Some were obtained from

moist lithium carbonate which had been fused into the aluminium cup. The blue line was very weak and nebulous, and it was difficult to obtain measurements of it. The following were obtained from four photographs :—4603·18, 4603·14, 4602·99 and 4603·97; the mean of these is 4603·15. Better photographs were obtained when a coil of wire was also introduced into the secondary circuit. The line more nearly resembled that obtained when no Leyden jar was used; the broad reversed line was more clearly defined at the electrode when negative than when positive.

An attempt was made to photograph the blue line in the Bunsen flame spectrum, but with an exposure of six hours no trace of it was obtained.

Other lines in the Spectrum of Lithium.

The orange line was examined in the arc and flame spectra by eye observations and by photography. The line in the flame spectrum was measured on four plates :—

Spectrum.	Wave-length.	
244^2	6103·84	centre of line.
244^2	6103·85	near tip of line.
244^4	6103·88	rather strong line.
245^2	6103·84	
245^3	6103·83	

The mean of these, omitting the rather strong line, is 6103·84. None of these lines showed any signs of reversal.

The wave-length of the bright line in the arc produced by the Gulcher lamp in the open was :—

Spectrum.	Wave-length.	
241^6	6103·81	nebulous in middle with sharp points. The nebulous part extended from 6102·46 to 6104·94.
242^4	6103·86	
242^5	6103·82	

The mean result is 6103·83.

The orange line is easily reversed in the arc. The reversed line was photographed and the wave-length measured on two plates, the results being 6103·82 and 6103·84. The reversal was narrow and the wings did not extend very far. The more refrangible wing was slightly broader than the other, and this difference was observed in the photographs and by eye observations. The part of the arc near the negative pole was examined very carefully; the line was most sharply reversed in that part, but there was no trace of a broad dark line. The orange line, with the exceptions noted, behaved normally.

Measurements were also made of two other lines in the arc spectrum. The wave-length of a weak and very diffuse line on one plate was 4132·82, and that of a very weak sharp line, 4273·32. The former line on another plate was stronger and broader; its middle was at 4132·35, whilst the other line, much broadened towards the red, had its strongest part at 4273·62.

The current used in working both the open and enclosed arcs was about 9 ampères. Kayser and Runge employed a current of 25—35 ampères from one machine and, to bring out the weak lines in some spectra, a current of 40—50 ampères from another machine. It seems probable that they worked with the intense arc which was only obtained by the author when the carbons were very near together, and that they observed only the spectrum of broadened lines which the author found was emitted by the intense arc and near the negative pole by weaker arcs. Their remarks on the appearances of the lines in the first and second subordinate series confirm this view. Eder and Valenta obtained a similar spectrum to Kayser and Runge's arc spectrum in the spark spectrum of metallic lithium when they used a condenser in the secondary circuit.

The author has pleasure in thanking Professor Liveing for the interest he has taken in this work and for some useful suggestions.

Conclusions.

The lines in the principal series of lithium appear to broaden and reverse normally.

The lines in the second subordinate series do not reverse even in the arc, but in strong arcs they broaden towards the less refrangible end of the spectrum and become diffuse on that side.

The first line in the first subordinate series, wave-length 6103·84, is almost normal; it broadens slightly more on the more refrangible side than on the other. The other lines in this series also broaden on both sides and become diffuse, but they broaden more rapidly on the more refrangible side than on the other. The centres of the broadened lines are more refrangible than the corresponding lines in the narrow state. The inner core of intense arcs, and the parts near the negative poles of weak arcs and sparks, give a broad reversed line with its centre about wave-length 4602·4; whilst the part near the positive pole in weak arcs, and the flame of the arc, give a sharp bright line, wave-length 4603·07, coincident with the lines in the spectra of the oxyhydrogen flame and uncondensed spark. The similar changes in the other lines diminish with their refrangibility. The wave-lengths hitherto recorded for these diffuse lithium lines would appear to be those of abnormal lines. The true lines are the sharp bright ones which occur, without complication, in the spectrum of lithium in the oxyhydrogen flame.

[ADDED NOV. 28, 1902.—Since this paper was communicated to the Royal Society, I have seen a paper on the spectrum of lithium, by Hagenbach, in the 'Annalen der Physik,' No. 12, 1902, which was published on November 13. The experimental part of his paper deals almost entirely with the blue line, and the fact that there are other abnormal lines in the spectrum of lithium is recorded above for the first time. Hagenbach's conclusion that there are two lines near wave-length 4603 is not, I think, established; and I still hold that the views expressed in this paper are more probable. He has not been able to find the second line as a bright line, so the difficulties in the way of accepting the view that there is a second dark line, without a corresponding bright line, remain. He has not referred to Professors Liveing and Dewar's work,* and his evidence for saying there are two lines is, in fact, similar to the evidence they gave.]

'An Error in the Estimation of the Specific Gravity of the Blood by Hammerschlag's Method, when employed in connection with Hydrometers." By A. G. LEVY, M.D. (London). Communicated by Sir VICTOR HORSLEY, F.R.S. Received November 25,—Read December 11, 1902.

(From the Laboratory of Pathological Chemistry, University College, London.)

Hammerschlag's method of estimation of the specific gravity of the blood is an application to clinical purposes of a physical method frequently employed when only a small quantity of the substance under investigation is obtainable. The method may be briefly described as the adjustment of the specific gravity of a mixture of chloroform and benzol by small successive additions of either constituent until it corresponds to the specific gravity of the blood, the test of the attainment of this condition being that a small drop of the blood, when immersed in the mixture, shall remain suspended without any very obvious tendency to rise or sink. The specific gravity of the mixture is then estimated by means of a hydrometer, the scale of which is graduated to register densities lying between the maximum and minimum densities of blood, *i.e.*, from 1·020 to 1·080.

In order to attain a rapid adjustment of the relative proportions of the chloroform and benzol, it is the general practice to use a comparatively small quantity only of these fluids and a small hydrometer, and, as will be hereafter seen, the size of the instrument is an important factor in the magnitude of the error.

This error was commented upon in a paper read by Dr. Baumann

* 'Phil. Trans.,' vol. 174 (1883), p. 215.

before a recent meeting of the Physiological Society, in which he recorded estimations by Hammerschlag's method which exceeded by 0·012 the estimation of the specific gravity of the blood by the picnometer method. Dr. Baumann also mentioned similar, but less considerable, excessive readings noted by certain other observers. I had myself, in the course of a series of experiments upon the blood of dogs,* occasion to remark upon the consistently high results yielded by Hammerschlag's method, the excess being, in my cases, from 0·007 to 0·008.

With the purpose of investigating the source of this error, I prepared two mixtures, the one (A) of chloroform and benzol, and the other (B) of glycerine and water, and, in each case, adjusted the relative proportions until identical readings were obtained on the scale of the same hydrometer. On immersing a drop of mixture (B) in a vessel of mixture (A), it rapidly sank to the bottom, thus indicating, in the absence of interfluid exchanges, an actually higher specific gravity of (B). This indeed could be demonstrated by other methods of finding the densities of the two liquids, *i.e.*, by the weighing bottle or picnometer, or by Westphal's specific gravity balance. The occurrence of a gross fault in the hydrometer method being established, it remained to investigate its extent and origin.

I proceeded by immersing four dissimilar hydrometers, all graduated from 1·000 to 1·060 to the test of immersion in a mixture of chloroform and benzol, which had been prepared, with the aid of Westphal's balance, of a specific gravity = 1·000, and the following table shows the point at which the lowest level of the fluid meniscus intersected each scale, *i.e.*, the *apparent* specific gravity for each hydrometer. The measurements of each instrument are included in the table for future reference.

The error, as set forth above, is explicable by a consideration of the differences between the values of the surface tensions of water and of chloroform and benzol. The exact experimental proof of this would, I find, involve an extended investigation into a complicated subject. This I have not attempted—it suffices to show how far the error, calculated from certain known conditions, agrees with my comparatively rough observations.

The action of surface tension of a fluid upon a floating hydrometer is evidenced in a downward pull upon the stem, so that a hydrometer becomes immersed, not only until it has displaced a weight of water equal to its own weight, but is still further immersed by the action of surface tension upon its stem until the additional weight of water displaced balances this surface tension pull. It is this point of ultimate

* "The Changes in the Blood of Dogs after Thyroidectomy," 'Journ. Path. and Bact. October 1898 p. 317.

Table I.

No. of hydrometer	No. 1.	No. 2.	No. 3.	No. 4.
Weight in grammes	15·3118	11·367	3·81	2·868
Diameter of stem in mm.....	3·95	4·37	3·55	3·13
Length of first division of scale (*i.e.*, 1·000 to 1·001) in mm.	1·29	0·74	0·417	0·398
Reading of scale in a chloroform benzol mixture of specific gravity = 1·000	1·002	1·003	1·0095	1·010

No. 1 hydrometer is a more sensitive instrument than the ordinary urinometer, having a large barrel and a comparatively fine stem. No. 2 is an ordinary urinometer such as is in general use in hospitals. Hydrometers Nos. 3 and 4 are considerably smaller instruments, and are similar to those which have been employed in this laboratory for use in connection with Hammerschlag's method.

immersion which corresponds to the mark 1·000 on the stem when the hydrometer is floating in water.

If the same hydrometer is floated in a chloroform and benzol mixture of sp. gr. = 1·000, the same volume of mixture is displaced, but, as in this case the liquid possesses a lower surface tension, the pull upon the stem is less powerful, and hence less of it is immersed from this cause than in the case of water, the degree of surface tension immersion being, in the two instances, in direct proportion to the respective values of the surface tensions. The mark 1·000 on the stem, therefore, floats a little distance above the surface of the mixture, and the hydrometer hence shows a reading which is higher than the actual specific gravity.

The length of the divisions and the diameter of the stem of any hydrometer being known, the error due to surface tension may be calculated.

The value of surface tension may be readily expressed in milligrammes for each millimetre of the circumference of the stem on which its acts. The surface tension of water is estimated by Van der Mensbrugghe as 7·3 milligrammes per mm. Other observers have found higher values, but it is difficult to obtain water sufficiently clean to exhibit even the surface tension of 7·3 milligrammes, for an exceedingly slight contamination of the surface by greasy matter suffices to appreciably reduce the tension.

The surface tensions of chloroform and benzol are very nearly equal

in value, and may be taken in each case, for purposes of calculation, as 2·75 milligrammes per mm., the actual figures given by some other observers varying slightly.*

I further find, by experiment, that the surface tension of a mixture of the two fluids of any specific gravity between 1·000 and 1·080 is, for practical purposes, the same as that of the individual fluids.

The numerical difference between the values of the surface tensions of water and a chloroform and benzol mixture is therefore 4·55 milligrammes (7·3—2·75), and this, when multiplied by the circumference (expressed in mm.) of the hydrometer stem, is equal to the weight in milligrammes of a column of the mixture of the same diameter as the stem in question, and of a length which equals the extent to which the stem is exposed below the specific-gravity mark which should be the proper reading of the hydrometer.

This length may be calculated according to the simplified formula $h = 2T/rw$, where h is height, T is surface tension, r is the radius of the stem, and w is the specific gravity of the fluid.

Having calculated this height in the case of a hydrometer immersed in a chloroform and benzol mixture of sp. gr. 1·000, the division of this by the average length of the first divisions of the scale gives the theoretical error (at this specific gravity) of the hydrometer, expressed in scale units.

Example.—In the case of Hydrometer No. 4,

$$h = \frac{2T}{rw} = \frac{2 \times 4{\cdot}55}{1{\cdot}565 \times 1} = 5{\cdot}8147$$

$$\frac{5{\cdot}8147}{0{\cdot}398} = 14{\cdot}6$$

As each division represents 0·001, the error = 0·0146.

In the following table, the calculated errors are contrasted with the observed errors of Table I.

Table II.

Hydrometer.	Observed error.	Calculated error.
1	0·002	0·0035
2	0·003	0·0056
3	0·095	0·0123
4	0·010	0·0146

The not inconsiderable disparity between the two columns was, for a time, an unsolved problem, until I found by the following experi-

* *Chloroform.*

Temp. 16°·6 C.	2·75	Quincke.
„ 12°·5 C.	2·813	Bedé.

Benzol.

Temp. 15° C.	2·87	Schiff.
„ 15° C.	2·76	Bedé.

ment that each instrument possessed an intrinsic error which tended to minimise the error of reading.

A hydrometer and its containing vessel were carefully cleansed with benzol and protected from contamination with greasy matter, and a sample of water taken, which was the cleanest readily obtainable, *i.e.*, tap-water which had been allowed to run through the pipe for some fifteen minutes. On immersing the hydrometer in this water, the mark 1·000 rested a slight distance below the surface. The water in which the hydrometer was standardised must have been contaminated, and hence possessed a considerably lower surface tension than that of the comparatively clean water in which my experiment was performed.

All the four hydrometers of my tables I found to possess this intrinsic error, which I estimated somewhat roughly, and have expressed in scale units. When these innate errors are added to the errors of Table I, the totals more closely approximate to the calculated errors. (See Table III.)

Table III.

Hydrometer.	Error due to difference between surface tensions of impure water and a chloroform-benzol mixture.	Error due to difference of surface tensions of impure and clean water.	Total error.	Calculated error.
1	0·002	0·0014	0·0034	0·0035
2	0·003	0·002	0·005	0·0056
3	0·0095	0·002	0·0115	0·0123
4	0·010	0·003	0·013	0·0146

There is thus sufficient agreement between the values of the observed and calculated errors to demonstrate that the disturbing influence of surface-tension is sufficient to cause the whole of the error in the hydrometer reading and to account for the inaccuracy of Hammerschlag's method. Taking into consideration the varying value of the surface tension of water, and the fact that no accurate determinations were made by me of each individual specimen of water or of chloroform and benzol, very exact calculations are precluded. Had this been done doubtless a more exact agreement between observations and calculations would have resulted. Furthermore, observations of this nature are replete with difficulties which can not be touched on here, but which may be gathered from a paper upon an elaborate investigation into a similar subject by Fridtjof Nansen.*

* "Scientific Results of the Norwegian North Polar Expedition," vol. 3, Part 10

The difference in the error of the several hydrometers is readily accounted for. A consideration of the facts already set forth shows that the error when expressed in scale units must vary directly as the radius of the stem, and inversely as the total weight of the instrument, so that when the stem is fine in comparison with the weight, the error is small. But in making a small hydrometer it is impossible to keep down the relative proportion of the stem. It thus follows that the smaller hydrometers exhibit a greater surface tension error than the larger ones.

The greatest discrepancy which I have *observed* in any hydrometer in chloroform and benzol was 0·014. This instrument was a small one, graduated from 1·020 to 1·080, and is not included in the above tables.

This source of error in Hammerschlag's method may be obviated by:—

(1.) The estimation of the specific gravity of the chloroform and benzol mixture by means of an instrument which excludes or minimises the surface tension factors. The most convenient is some such balance as Westphal's, in which the surface of the fluid is intersected by an exceedingly fine platinum wire only. The employment of hydrostatic bubbles is inconvenient on account of the long series required. The picnometer method is not readily applicable in connection with very volatile fluids.

(2.) By employing a hydrometer which has been standardised or corrected in chloroform and benzol mixtures, the requisite specific gravities of which have been adjusted by an accurate method. In the absence of the above-mentioned appliances a rough method or correction may be applied to any hydrometer of which the highest mark is 1·000. The method consists of adjusting the proportions of a mixture of chloroform and benzol until a small drop of water immersed neither sinks or floats. The mixture being thus of the same specific gravity as water itself, the reading of the hydrometer in it is its error at this degree, and in the case of a well-constructed and accurately graduated hydrometer, this error holds good with only a negligible increase throughout the scale.

I have, in conclusion, to express my indebtedness to Professor Vaughan Harley for the resources of his laboratory, and to Professors Baly and Donnan, of the Chemical Department, at University College, for kind assistance and the loan of appliances.

"Quaternions and Projective Geometry." By CHARLES J. JOLY, F.T.C.D., Royal Astronomer of Ireland. Communicated by Sir ROBERT S. BALL, F.R.S. Received November 27,—Read December 11, 1902.

(Abstract.)

The object of this paper is to include projective geometry within the scope of quaternions. The calculus, as established by Hamilton, was solely adapted to the treatment of metrical relations, but when we regard a quaternion as representing a weighted point, projective properties can be investigated with great facility. Writing

$$q = \left(1 + \frac{Vq}{Sq}\right) Sq,$$

the point represented by the quaternion, q, is the extremity of the vector, Vq/Sq, drawn from an arbitrary origin, and the weight attributed to the point is Sq;* and this interpretation requires no modification in the principles of the calculus.

In this paper the theory of the linear quaternion function is developed to a considerable extent, and this theory is of fundamental importance, because the most general homographic transformation in space is expressible by means of a linear quaternion function. One section of the paper is devoted to the consideration of the scalar invariants of linear quaternion functions, and among these are included the invariants of systems of quadrics which correspond to the particular case in which the functions are self-conjugate. Moreover, in this section and more fully in the section on *covariance*, it is pointed out that the invariance in the case of the general functions is wider than in the case of self-conjugate functions. In fact, in the special case, the functions must remain self-conjugate after transformation. It is shown that there are in all eight distinct types of covariance.

The decomposition of linear transformations is also considered, and much use is made of the square-root of a linear quaternion function. A section is occupied with the determination of the linear transformations which shall convert given figures into other given figures, and with the conditions which in certain cases must be obeyed in order that such a transformation may be possible.

The general surface, the principle of reciprocity, generalised curvature and geodesics, are dealt with in a subsequent section, and the chief properties of an operator analogous to Hamilton's ∇ are exhibited in the section immediately following.

Several sections are occupied with the theory of the bilinear quater-

* 'Trans. R. Irish Acad.,' vol. 32, pp. 1--16.

nion function, and this function is employed in the investigation of the properties of a four-system of linear transformations, of the general quadratic transformation, and of the non-linear one-to-one correspondence of points in space. The method of quaternion arrays* is applied to the discussion of n-systems of linear transformations, and of the critical assemblages of points, lines and planes connected with each system of transformations. Finally, in the concluding section it is explained how the method of the paper may be applied to hyper-space, or to the discussion of functions of any number of variables; and in many cases the formulæ obtained in the course of the paper with special reference to three dimensions require no modification to fit them for the general case of n variables.

"The Stability of the Pear-shaped Figure of Equilibrium of a Rotating Mass of Liquid." By G. H. Darwin, F.R.S. Plumian Professor and Fellow of Trinity College, in the University of Cambridge. Received and Read June 19, 1902.

(Abstract.)

At the end of a previous paper† it was stated that the stability of the pear-shaped figure could not be definitely proved without further approximation. After some correspondence with M. Poincaré during the course of my work on that paper, I attempted to carry out the second approximation, but found myself foiled at a certain stage of the work. Meanwhile he had turned his attention to the subject, and he has‡ shown how the difficulty which stopped me may be overcome. He has not, however, pursued the arduous task of converting his results into numbers, so that he leaves the question of stability unanswered.

M. Poincaré was so kind as to allow me to detain his manuscript on its way to the Royal Society for a few days, and being thus assisted I was able to resume my attempt under favourable conditions, and this paper is the result. The substance of the analysis of this paper is, of course, essentially the same as his, but the two present but little superficial resemblance. It is perhaps well that the two investigations of so complicated a subject should be nearly independent of one another.

If a mass of liquid be rotating like a rigid body with uniform angular velocity, the determination of the figure of equilibrium may be

* 'Trans. R. Irish Acad., vol. 32, pp. 17—30.

† 'Phil. Trans.,' A, vol. 198, pp. 301—331.

‡ 'Phil. Trans.,' A, vol. 198, pp. 333—373.

treated as a statical problem, if the mass be subjected to a rotation potential. The energy lost in the concentration of such a system from a condition of infinite dispersion consists of two parts. The first of these, say W, is the lost energy of the system at rest; the second is equal to the kinetic energy, say T, of the system in motion. The whole lost energy, say E, is equal to $W + T$, and the condition for a figure of equilibrium is that E shall be stationary for all variations, subject to constant angular velocity.

It might appear at first sight that the condition for secular stability is that E shall be a maximum. But M. Poincaré has shown that this condition is insufficient, and that it is necessary for stability that the whole energy, say U, which is equal to $-W + T$, shall be a minimum for all variations, subject to the condition of constancy of angular momentum.

He has, however, adduced another consideration, which enables us to determine the stability from the variations of E, without a direct consideration of the function U. He has shown, in fact, that if for given angular momentum slightly less than that of the critical Jacobian ellipsoid, from which the pear-shaped figures bifurcate, there is only one possible figure, namely, the Jacobian; and if for slightly greater angular momentum there are two figures, namely, the Jacobian and the pear,* then exchange of stability between the two series must occur at the bifurcation. If, on the other hand, the smaller momentum corresponds with the two figures and the larger with only one, one of the two (namely, the Jacobian) must be stable, and the other (namely, the pear) unstable.

The question is then completely answered by the value of the momentum of the pear; if it is greater than that of the critical Jacobian, the pear is stable, and if less, unstable. It suffices then to determine the pear from the variations of E with constant angular velocity, and afterwards to evaluate the angular momentum.

In the first approximation the pear-shaped figure is represented by the third zonal harmonic inequality with reference to the longest axis of the critical Jacobian ellipsoid. In proceeding to the higher approximation I suppose that its amplitude is measured by a parameter e, which is to be regarded as a quantity of the first order. We must now also suppose the ellipsoid to be deformed by every other harmonic, but with amplitudes of order e^2. In the first approximation W was proportional to e^2, but it now becomes necessary to go as far as the order e^4. A change in the sign of e means that the figure is rotated in azimuth through 180°. As this rotation cannot affect the energy, the odd powers of e must be absent from the expression for W. We have further to find the moment of inertia, as far as the terms

* For the sake of simplicity, I speak of one pear instead of two in azimuths differing by 180°.

of order e^2, and thence to find the kinetic energy T. The function F is then equal to W + T.

In order to attain the requisite degree of accuracy it is convenient to regard the pear as being built up in an artificial manner.

I construct an ellipsoid similar to and concentric with the critical Jacobian, and therefore itself possessing the same character. The size of the new ellipsoid, which I call J, is undefined; and is subject only to the condition that it shall be large enough to enclose the whole pear. The region between J and the pear being called R, I suppose the pear to consist of positive density throughout J and negative density throughout R.

The lost energy of the pear consists of that of J with itself, say $\frac{1}{2}$JJ; of J with R, which is filled with negative density, say $-$JR; and of $-$R with itself, say $\frac{1}{2}$RR. This last contribution (which had baffled me) must be broken into several parts.

If we imagine J to be intersected by a family of orthogonal curves, and if we suppose for the moment that the region R is filled with positive matter, we may further imagine the matter lying inside any orthogonal tube to be transported along the tube, and deposited on the surface of J in the form of a concentration of positive surface density +C.

In the actual system R is filled with negative density, and we may clearly add to this two equal and opposite surface densities +C and $-$C on J. The matter lying in the region R may then be regarded as consisting of negative surface density $-$C, together with a double system, namely negative volume density $-$R, conjoined with equal and opposite surface density +C. This double system, say D, is therefore C $-$ R.

The lost energy $\frac{1}{2}$RR may now be considered as consisting of three parts, first, the energy of $-$C with itself, say $\frac{1}{2}$CC; secondly, that of D with itself, say $\frac{1}{2}$DD; and thirdly of $-$C with D. This third item is obviously equal to $-$CC + CR, and therefore $\frac{1}{2}$RR is equal to $-\frac{1}{2}$CC + CR + $\frac{1}{2}$DD. Thus W, the gravitational lost energy of the pear, may be written symbolically—

$$\tfrac{1}{2}\text{JJ} - \text{JR} + \text{CR} - \tfrac{1}{2}\text{CC} + \tfrac{1}{2}\text{DD}.$$

In this discussion no attention has as yet been paid to the rotation, but fortunately it happens that the introduction of this consideration actually simplifies the problem, for if we suppose $\frac{1}{2}$JJ and JR to mean the lost energies of J with itself and with R on the supposition that the mass is rotating with the angular velocity of the critical Jacobian, the formulæ become much more tractable than would otherwise have been the case.

The inclusion of part of the angular velocity in this part of the function only leaves outstanding the excess of the kinetic energy of

the pear above the kinetic energy which it would have had if it rotated with the angular velocity of the critical Jacobian. If ω denotes the latter angular velocity, and $(\omega^2 + \delta\omega^2)^{\frac{1}{2}}$ the actual angular velocity of the pear; if A_j, A_r denote the moments of inertia of J, and of R considered as filled with positive density, we have

$$E = \tfrac{1}{2}JJ - JR + CR - \tfrac{1}{2}CC + \tfrac{1}{2}DD + \tfrac{1}{2}(A_j - A_r)\,\delta\omega^2.*$$

The co-ordinates of points are determined by reference to the ellipsoid J which envelopes the whole pear. The size of J is indeterminate, and therefore the formulæ must involve an arbitrary constant expressive of the size of J. But the final result for E cannot in any way depend on the size of the ellipsoid which is chosen as the basis for measurement, and therefore the arbitrary constant must ultimately disappear. Hence it is justifiable to treat it as zero from the beginning, and we may use the formula for the internal gravity throughout the investigation.†

Although the constant expressive of the size of J is put equal to zero—which means that the pear is really partly protuberant beyond the ellipsoid—yet there is a considerable amount of mental convenience in continuing to discuss the subject as though the ellipsoid completely enveloped the pear.

When an ellipsoid is deformed by an harmonic inequality, the volume of the deformed body is only equal to that of the ellipsoid, to the first order of small quantities. In the case of the pear, all the inequalities, excepting the third zonal one, are of the second order, and as far as concerns them the volumes of J and of the pear are the same. But it is otherwise as regards the third zonal harmonic term, and the first task is to find the volume of such an inequality as far as e^2. When this is done, we can express the volume of J in terms of that of the pear, which is of course a constant.

By aid of ellipsoidal harmonic analysis we may now express the first four terms of E in terms of the mass of the pear and of certain definite integrals which depend on the shape of the critical Jacobian ellipsoid.

The energy $\frac{1}{2}DD$ presents much more difficulty, and it is especially in this that M. Poincaré's insight and skill have been shown. The system D consists of a layer of negative volume density coated on its outer surface with a layer of surface density of equal and opposite mass. His procedure virtually amounts to regarding this system as consisting of an infinite number of magnetic layers, whose energy may be evaluated and summed. The reduction of this part of the energy to calculable forms is not very simple.

* A term depending on the shift of the centre of inertia proves to be negligible.

† Compare with M. Poincaré's treatment of the same point, 'Phil. Trans.,' A, vol. 198, p. 352.

The moment of inertia of the pear presents but little difficulty, since it only involves those harmonic inequalities of J which are expressible by harmonics of the second degree. On multiplying the moment of inertia by $\frac{1}{2}\delta\omega^2$ we obtain the last contribution to the expression for E.

The portion of E independent of $\delta\omega^2$ cannot involve e^2, since the vanishing of the coefficient of that term is the condition whence the critical Jacobian ellipsoid was determined. If f denotes the coefficient of any harmonic inequality other than the third zonal one, this portion of E is found to contain terms in e^4, e^2f, and $(f)^2$. The coefficient of $\delta\omega^2$ consists of a constant term and terms in e^2, f_2, f_2^2, where these f's denote the coefficients of the second zonal and sectorial harmonics. If f refers to any harmonic of odd degree, the coefficient of the corresponding term in e^2f vanishes. If, then, we make E stationary for variations of the coefficient of any odd harmonic, that coefficient is seen to vanish. Hence it follows that the expression for the pear cannot involve any odd harmonic other than the third zonal one. Conditions of symmetry also negative the existence of even harmonics of the sine type, and of even harmonics of the cosine type but of odd rank.

On equating to zero the variations of E for all the remaining fs, excepting f_2 and f_2^2, we at once obtain their values in terms of e^2. Equating to zero the variations for e^2, f_2, f_2^2, we obtain three equations, which give $\delta\omega^2$, f_2, f_2^2 as multiples of e^2.

It seems unnecessary to explain here the methods adopted for reducing the analytical results to numbers; it may suffice to say that the task was very laborious.

The harmonic terms included in the computation were those of degree 2, and ranks 0, 2; of degree 4, and ranks 0, 2, 4; and of degree 6, and ranks 0, 2, 4. The sixth sectorial harmonic would certainly have proved negligible.

The expression for $\delta\omega^2$ was found in the form of a fraction, of which the denominator is determinate, and the numerator is the sum of an infinite series. Nine terms of this series were computed, namely, a constant term and the contributions of the eight harmonics above enumerated. The result shows that the square of the angular velocity of the pear is less than that of the Jacobian in about the proportion $1 - \frac{1}{8}e^2$ to 1.

On the other hand, the angular momentum is greater in about the proportion of $1 + \frac{1}{15}e^2$ to 1. If this last result were based on a rigorous summation of the infinite series, it would absolutely prove the stability of the pear. The inclusion of the uncomputed residue of the series would undoubtedly tend in the direction of reducing the coefficient given above in round numbers as $\frac{1}{15}$, and if it were to reduce it to a negative quantity we should conclude that the pear is unstable after all.

The apparently rapid convergence of the series seemed to render such a reversal of the result almost incredible. In order, however, to feel yet more sure, I made a rough estimate of the contribution of the eighth zonal harmonic, and found that it would only amount to $\frac{1}{175}$th part of that critical total which would just show the pear to be unstable.

Since the convergency of the series is obviously rapid, I regard it as proved, but by something short of absolute algebraic proof, that the pear is stable.

The numbers obtained in the course of the work afford the means of giving a second approximation to the form of the pear, and the result is shown in figures, drawn with the largest value of e, which seemed consistent with a fair degree of approximation.

I originally called the figure "pear-shaped" because M. Poincaré's conjectural sketch in the 'Acta Mathematica' was very like a pear. In the first approximation, shown in my former paper, the resemblance to a pear was not striking, and it needs some imagination to see the pear-shape in the new figures; but a distinctive name is so convenient that we may as well continue to call it by that name.

The effects of the new terms are almost entirely concentrated at the two ends. They tend to augment the protuberance of the stalk end, and to diminish the depression at the blunt end so much as nearly to fill it up. Over the greater part of the figure the depressions and protuberances are less conspicuous than they were.

I think it is hardly too much to say that in a well-developed "pear" the Jacobian ellipsoid has nearly regained its primitive figure, but that it has shot forth a protuberance at one end. A consideration of the figures and of a conjectural extension of them almost reminds one of some such phenomenon as the protrusion of a filament of protoplasm from a mass of living matter. Notwithstanding the warning of M. Poincaré as to the danger of applying these results to heterogeneous masses and thence to cosmogony, I cannot restrain myself from joining him in seeing in this almost life-like process a counterpart to at least one form of the birth of double stars, planets, and satellites.

"On the 'Blaze-currents' of the Incubated Hen's Egg." By AUGUSTUS D. WALLER, M.D., F.R.S. Received July 17,—Read December 4, 1902.

(From the Physiological Laboratory of the University of London, S.W.)

In previous communications to the Society, on the Eyeball,* on the Skin,† and on Leguminous Seeds,‡ I have reported the results of experiments conducted by aid of an electrical criterion distinguishing between the living and not-living state.

The present communication contains the results of a series of systematic observations on the hen's egg by aid of the same distinguishing test—or blaze reaction, as I was led to designate it when it first came under my observation in the case of the frog's eyeball.

The case of the hen's egg is particularly interesting, for while we cannot tell *a priori* with any assurance whether or no a dormant egg will give the reaction characteristic of living matter, we may—after having learned by experience that it does *not* do so—expect to find the reaction make its appearance with the progress of development by incubation. And as a matter of fact, we find that this is what happens.

I first tested several eggs of unknown origin, bringing electrodes into contact with the superior and inferior extremities of a vertical short diameter of the egg laid upon its side. On account of the resistance of the shell, a small piece was removed on each side, and the electrodes brought into contact with the subjacent unbroken shell-membrane. There was no blaze in either direction, but only slight polarisation counter-currents of 0·0001 to 0·0002 volt. A similar result was obtained when the yolk was laid bare and the superior electrode applied directly to the cicatricula, both with the egg at ordinary room-temperature (20°) and in an incubator at 37°.

I then proceeded to test a series of ten fresh eggs reputed "fertile" and fit for incubation.

The results were briefly as follows:—

Egg No. 1, at the end of 24 hours' incubation, gave a small ascending blaze.

Egg No. 2, at the end of 48 hours, gave a similar but rather more distinct effect.

* "On the Blaze-currents of the Frog's Eyeball," 'Phil. Trans.,' B, vol. 194, 1901.

† "On Skin Currents. Part I. The Frog's Skin," 'Roy. Soc. Proc.,' June 6, 1901; "Part II. Observations on Cats," *ibid.*, November 21, 1901; "Part III. The Human Skin," *ibid.*, May, 1902.

‡ "An Attempt to Estimate the Vitality of Seeds by an Electrical Method," 'Roy. Soc. Proc.,' vol. 68, p. 79.

Egg No. 3, also at the end of 48 hours, gave large effects in both directions, larger and more persistent in the upward than in the downward direction. The difference between the reactions of these two eggs was in obvious correlation with their unequal degree of development, for whereas in No. 2 an area vasculosa was only just apparent at one border, in No. 3 it was well formed, and the heart was observed pulsating for more than 12 hours after exposure of the blastoderm.

No. 4 (72 hours) reacted well in both directions, and was normal.

No. 5 (72 hours) gave me pause. In spite of repeated trial I could not obtain a trace of the reaction that I expected to obtain. Every stimulus of whatever strength and direction gave rise to a slight counter-effect. But the explanation of the result was forthcoming when the egg was opened. No development whatever had taken place.

No. 6 (72 hours) gave normal reaction in both directions. Development was normal.

No. 7 (96 hours), normal reactions and normal development. Reaction abolished by rise of temperature with embryo exposed.

No. 8 (108 hours), normal reactions and normal development. Reaction abolished by rise of temperature.

No. 9 (144 hours), normal reactions and normal development. Reaction abolished by injection of a 2·7 per 100 solution of mercuric bichloride.

No. 10 (12th day of incubation) gave no blaze in either direction, only polarisation. The contents of the egg were rotten.

The above series of results was evidently in accordance with the fundamental fact. There was no exception to rule in any of the ten trials.

Certain of these ten observations were taken in closer detail in order to get at information as to relation between stimulus and response, effect of strong electrical stimulation, &c., and although these matters will demand considerable further investigation, some of the results may be described now.

This first series, by no means satisfactory from a chicken-farmer's point of view, was practically conclusive for my purpose, which was to learn whether the presence or absence of a living embryo could be diagnosed from the presence or absence of blaze-currents.

Other trials made at times of year still more unfavourable as regards probability of development, viz., in August and in November, gave results that were equally satisfactory from my point of view.

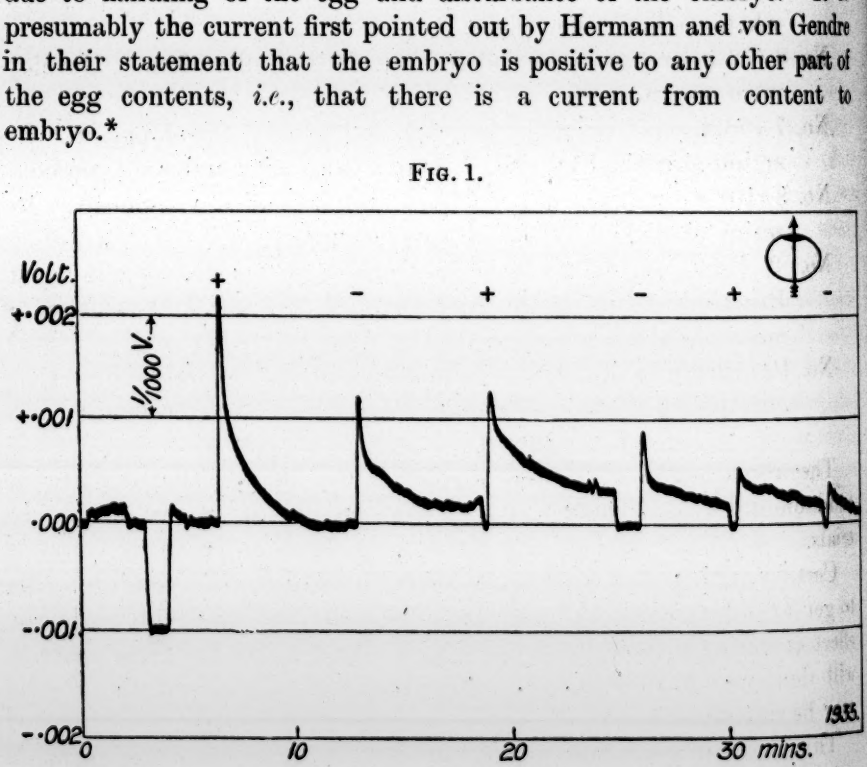
Thus in the second series, No. 1 after 32 hours' incubation gave small blaze effects of ± 0·0010 volt in both directions; the blastoderm was the size of a sixpence, and there was no sign of circulation. Nos. 2

and 3 were used in the absence of incubation for a careful examination of the exposed blastoderms, one at normal temperature, the other at 38°; polarisation currents were alone observable in these two cases, amounting to ± 0·0002 volt. Nos. 4, 5, and 6 at 50 and 58 hours gave positive blaze of only 0·0030 and 0·0010 volt, and their blastoderms were found to be very defective. Nos. 6, 7, 8, 9, and 10 gave no blaze whatever, only the usual small counter-deflections of ± 0·0002 volt due to polarisation, and on being opened exhibited no sign of development.

Incidentally to this first series of trials I noted that:—

1. The "normal current," from a developing egg, led off as described, is positive or ascending, and if the egg is left undisturbed diminishes during observation. I consider it to be a "manipulation blaze" due to handling of the egg and disturbance of the embryo. It is presumably the current first pointed out by Hermann and von Gendre in their statement that the embryo is positive to any other part of the egg contents, *i.e.*, that there is a current from content to embryo.*

FIG. 1.



Hen's Egg after 48 hours' incubation. Coil at 0. Single break shocks in ascending (+) and descending (−) directions. Response only in ascending (+) direction.

2. The embryo is easily exhausted and killed by repeated excitations of moderate and of excessive strength. Fig. 1 (No. 1935) exhibits the fatigue decline of a succession of reactions to strong induction

* Hermann u. von Gendre, "Ueber eine Electromotorische Eigenschaft des bebrüten Hühnereies," 'Pflüger's Archiv,' 1885.

shocks at intervals of 5 or 6 minutes. By strong tetanisation the reaction is completely abolished; the chick has been "electrocuted."

In a third series of ten trials upon "fresh eggs" from a London shop, and at a very unfavourable time of year as regards development, the results were as clear as could be desired. At the end of 12 days of incubation nine of these eggs returned what was by this time familiar to me as a negative answer, viz., small counter-currents due to polarisation, and all the nine showed no sign of development; only a single egg gave blaze-currents of $\pm 0{\cdot}0010$ volt, and was found to have developed to the extent usual at the end of 24 hours under ordinarily favourable conditions.

In the following year (1902) I returned to the subject and made two further series of observations, paying particular attention to the direction of blaze-currents of the first few days of incubation. In the interval between these observations and those of the previous year I had studied the currents of mucous membranes, with the general, but by no means invariable, result that the blaze-currents are of ingoing direction. I therefore expected to find, and did find, that the response of an early blastoderm to either direction of current is of positive or ascending direction, *i.e.*, ingoing as regards the hypoblast, and outgoing as regards the epiblast.

I also took the opportunity of testing active eggs on what has been described in previous papers* as the A B C plan, *i.e.*, after excitation through A, the superior pole, and B, the inferior pole of the egg, the lead-off to the galvanometer through A C was found to be effective (outgoing current at A), and the lead-off through B C ineffective. The response was outgoing or positive at A after both directions of excitation between A B.

The method of observation is further illustrated by the following table and plates. Plates 1939–40 taken on the 6th egg (72 hours) are given in detail to show how the magnitude of response varies with

1939–40. Chick Embryo. 72 hours.

Arithmetic Increase of Quantity and of Energy by increasing Capacity at Constant Voltage.

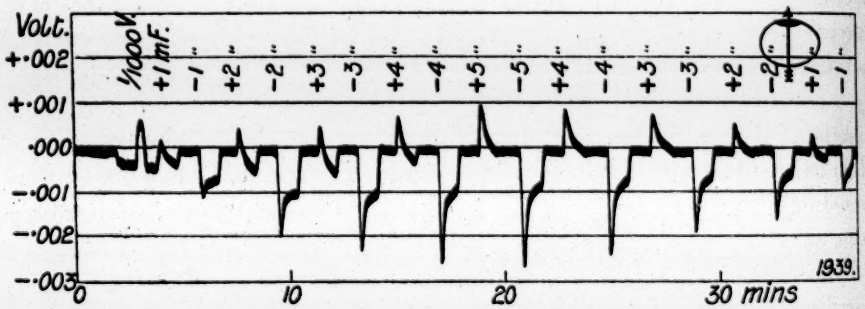
Capacity.	Pressure.	Quantity.	Energy.	Response.
1 mf.	8·4 volts.	8·4 mc.	360 ergs.	0·0010 volt.
2 „	8·4 „	16·8 „	720 „	0·0018 „
3 „	8·4 „	25·2 „	1080 „	0·0022 „
4 „	8·4 „	33·6 „	1440 „	0·0025 „
5 „	8·4 „	42·0 „	1800 „	0·0027 „

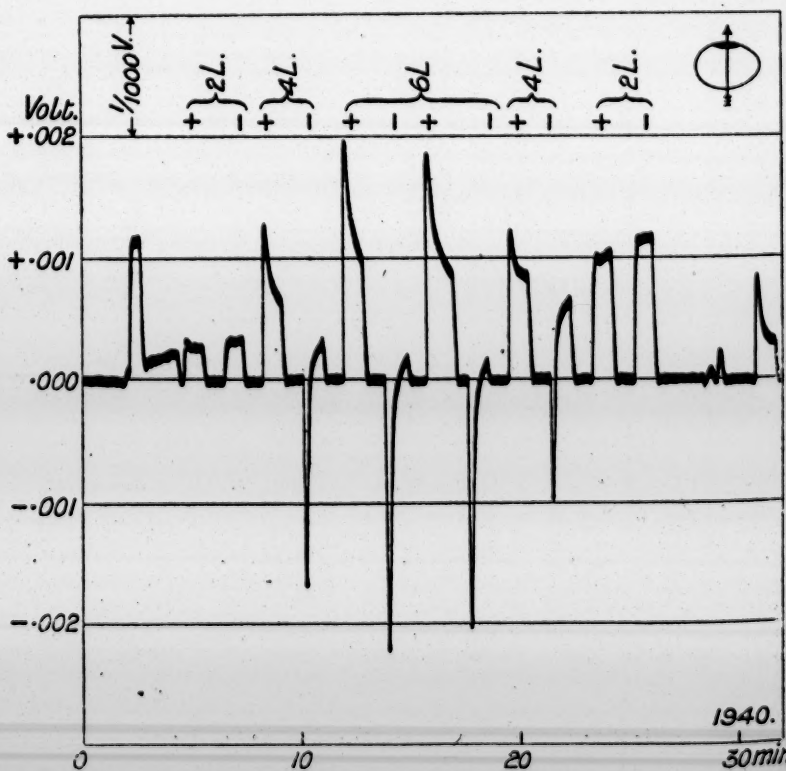
* Roy. Soc. Proc.,' vol. 68, p. 488; vol. 69, p. 181.

the magnitude of its exciting cause. The response appears to depend on energy rather than on quantity of electrical stimulus; plate 1948 taken on the 8th egg (108 hours) shows this point even more clearly. The same chick was used for examination of the influence of temperature.

Arithmetic Increase of Quantity and Geometric Increase of Energy by increasing Voltage at Constant Capacity.

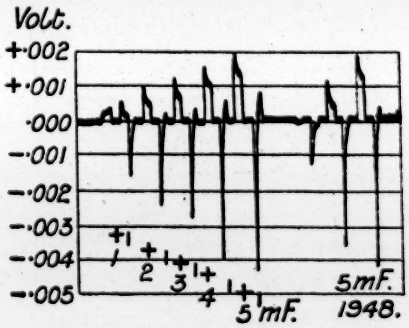
5 mf.	2·8 volts.	14 mc.	200 ergs.	0·0005 volt.
5 „	5·6 „	28 „	800 „	0·0011 „
5 „	8·4 „	42 „	1800 „	0·0013 „





Chick, 108 hours.

Pressure.	Capacity.	Quantity.	Energy.	Response.
6 L.	1 mf. +	8·4 mc.	360 ergs.	+0·0005
,,	,, −	..	..	−0·0016
,,	2 mf. +	16·8	720	+0·0009
,,	,, −	..	..	−0·0025
,,	3 mf. +	25·2	1080	+0·0011
,,	,, −	..	..	−0·0032
,,	4 mf. +	33·6	1440	+0·0015
,,	,, −	..		−0·0040
,,	5 mf. +	42·0	1800	+0·0019
,,	,, −	..	..	−0·0043
2 L.	5 mf. +	14·0	200	+0·0000
,,	,, −	..	..	−0·0015
4 L.	,, +	28·0	800	+0·0010
,,	,, −	..	..	−0·0036
6 L.	,, +	42·0	1800	+0·0019
,,	,, −	..	..	−0·0042



July 25. Chick, 108 hours.

Influence of Temperature.

Exc. by Condenser discharge. 5·6 volts; 4 L 5 mf. +

Time.	Temp.	Blaze.	Resistance.
0	28°	+0017	50,000 ω
5	28	0016	—
10	34	0014	—
15	38·5	0009	—
20	41·5	0004	35,000
25	44°	0000	—
35	37	0000	—
45	31	0000	35,000

Summary.

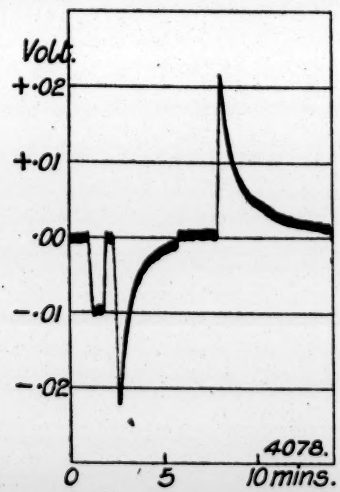
The presence of a blaze-current is a certain sign that development has progressed within the egg.

In the early stages—when presumably the blastodermic membrane has not yet become folded to form a tubular embryo—the blaze-currents aroused by both directions of excitation are positive or ascending.

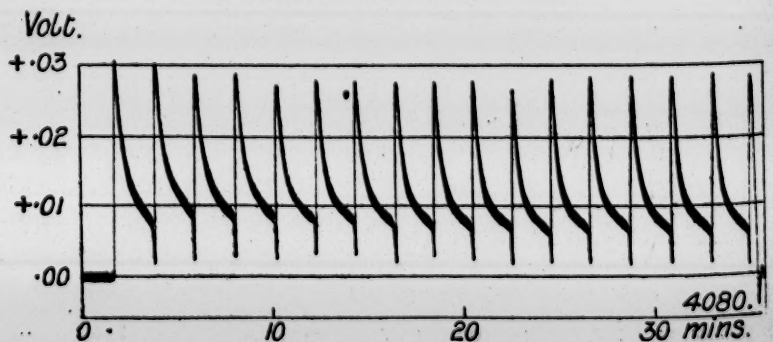
At a more advanced stage of development the blaze-currents are usually homodrome with the direction of excitation, viz., positive or ascending after a positive or ascending excitation, and negative or descending after a negative or descending excitation.

In some cases, but with such infrequency as to deserve to be characterised as exceptional, both responses have been observed to occur in a negative or descending direction. This may have been due to attachment of the embryo to the shell.

[I have made a few observations on frog's spawn, and although these have not yet been sufficient to enable me to specify the conditions of the presence or absence of the reaction, I think that the fact of its presence is worth reporting, as well as the further fact that in some cases of undoubtedly living spawn I have failed to detect it.]



Frog's Spawn.—Two homodrome responses of + and − 0·022 volt to excitation by break current at + and − 5000.



Frog's Spawn.—Series of homodrome responses to series of single break induction currents 5000 + at intervals of two minutes.

Fourth Series. June 1902. Berne Coil. 2 Leclanché's. 10,000 units.

Fourth Series. June 1902. Berne Coil. 2 Leclanché's. 10,000 units.

			Single break induction shocks.		Tetanising currents.		State found on opening the egg.
			br. +	br. −	m − br +	m + br −	
1st egg	Shell broken	46 hours ..	+0·0020	+0·0010	+0·0055	+0·0030	Normal.
2nd ,,	,, unbroken	46 ,, ..	−0·0030	+0·0010	+0·0030	+0·0025	Normal.
	,, broken	46 ,, ..	+0·0025	+0·0014	+0·0040	+0·0015	Normal.
3rd ,,	,, ,,	46 ,, ..	+0·0003	+0·0002	+0·0006	+0·0005	Normal. (Strong tetanisation before test was made.)
4th ,,	,, unbroken	46 ,, ..	−0·0007	−0·0002	—	—	Doubtful result.
	,, broken	..	+0·0012	+0·0005	+0·0040	+0·0022	Normal.
5th ,,	,, ,,	66 ,, ..	+0·0025	+0·0015	+0·0090	+0·0045	Normal. Cirra vasculosa well-developed.
6th ,,	,, ,,	90 ,, ..	+0·0040	+0·0010	+0·0120	+0·0035	Normal.
7th ,,	,, unbroken	4th day ..	+0·0022	+0·0010	+0·0100	+0·0006	Strong tetanisation (8 Lecl. 10,000).
	Same egg	8th ,, ..	trace −	trace +	+0·0005	−0·0005	Embryo dead and smelling—killed presumably by strong tetanisation.
8th ,,	Shell broken	5th ,, ..	+0·0030	+0·0007	+0·0125	+0·0050	Normal.
	Egg boiled	..	−0·0002	+0·0024			
9th ,,	Shell unbroken	6th ,, ..	+0·0003	+0·0001	+0·0010	+0·0000	Doubtful result.
	,, broken	..	? trace	? trace	? trace	? trace	No sign of development. Bad smell.
10th ,,	,, ,,	7th ,, ..	+0·0008	−0·0002	+0·0030	+0·0020	Normal.
11th ,,	,, ,,	9th ,, ..	nil	nil	nil	nil	Embryo dead and smelling.
12th ,,		10th ,, ..	−0·0010	−0·0015	−0·0030	−0·0060	Well developed chick. All responses in descending direction. All responses of both eyes were positive, +0·0010 to +0·0015.
13th ,,	,, ,,	3rd ,, ..	nil	nil	trace +	trace −	No development.
14th ,,	,, ,,	3rd ,, ..	+0·0060	+0·0018	+0·0060	+0·0020	Normal.
15th ,,	,, ,,	3rd ,, ..	+0·0006	+0·0002	+0·0016	+0·0002	Normal.
16th ,,	,, ,,	5th ,, ..	nil	nil	nil	nil	No development. Bad smell. N.B. These eggs had not been turned or kept moist in the incubator for 3 or 4 days.
17th ,,	,, ,,	5th ,, ..	nil	nil	nil	nil	
18th ,,	,, ,,	5th ,, ..	nil	nil	nil	nil	
19th ,,	,, ,,	5th ,, ..	nil	nil	nil	nil	
20th ,,	,, ,,	5th ,, ..	nil	nil	nil	nil	

The numbers in these four columns signify voltage of response.

First Series (1901).

			Excitation.	Response.	
Unrec.	July 19	Hen's egg (from shop).....	6 L., 5 mf. ±	nil ..	Only polarisation counter-currents in both directions. Same with exposed yolk.
	,, ,,		Br O ±	nil ..	
	,, 22	1st egg (24 hours incubated)	Br O + ...	small + ..	Small ascending blaze.
	,, ,,		Br O −	nil ..	
	,, 23	2nd egg (48 hours)	Br O + ...	small + ..	Scarcely developed vascular area. Small ascending blaze.
	,, ,,		Br O −	nil ..	
1934	,, ,,	3rd egg (48 hours)	Br O + ...	+0·0010 volt ..	Well-developed vascular area. Pulsating cardiac speck. Ascending blaze.
	,, ,,	,, ...	Br O −	+0·0006 ,, ..	
1935	,, ,,	,, (60 hours)	Br O + ...	+0·0022 ,, ..	Pulsation still visible. 12 hours later. Ascending blaze.
	,, ,,	,, ...	Br O −	+0·0011 ,, ..	
1936	,, ,,	,, ...	Br O −	+0·0005 ,, ..	4 hours later. Pulsation just visible.
	,, ,,	,, ...	Br O + ...	+0·0010 ,, ..	
Unrec.	,, 24	4th egg (72 hours)	Br O + ...	+ large	Response in both directions. Well-developed embryo.
	,, ,,	,, ...	Br O −	− large	
,,	,, ,,	5th egg (72 hours)	6 L., 5 mf. +	− small	No development. No blaze. Only polarisation.
	,, ,,	,, ...	6 L., 5 mf. −	+ small	
	,, ,,	,, ...	Br O + ...	− small	
	,, ,,	,, ...	Br O −	+ small	
1939	,, ,,	6th egg (72 hours)	6 L., 5 mf. +	+0·0010 volt ..	Good vascular area and embryo. (*In extenso.*) Ascending and descending blaze.
	,, ,,	,, ...	6 L., 5 mf. −	−0·0026 ,, ..	
1940	,, ,,	,, ...	6 L., 5 mf. +	+0·0019 ,, ..	
	,, ,,	,, ...	6 L., 5 mf. −	−0·0023 ,, ..	
1941	,, 25	7th egg (96 hours)	6 L., 5 mf. +	+0·0028 ,, ..	Good embryo. Ascending and descending blaze.
	,, ,,	,, ...	6 L., 5 mf. −	−0·0044 ,, ..	Temperature raised to 40°. Reactions abolished.
1942	,, ,,	,, ...	6 L., 5 mf. +	+0·0015 ,, ..	
	,, ,,	,, ...	6 L., 5 mf. −	−0·0045 ,, ..	
1943	,, ,,	,, ...	6 L., 5 mf. +	nil ..	
	,, ,,	,, ...	6 L., 5 mf. −	nil ..	
1948	,, ,,	8th egg (108 hours)	4 L., 5 mf. +	+0·0010	Ascending and descending blaze. (*In extenso.*) Temperature raised to 44°. Reaction abolished.
	,, ,,	,, ...	4 L., 5 mf. −	−0·0036	
Unrec.	,, ,,	,, ...	4 L., 5 mf. +	nil ..	
	,, ,,	,, ...	4 L., 5 mf. −	nil ..	
1949	,, 27	9th egg (144 hours)	6 L., 5 mf. +	+0·0025	Ascending and descending blaze before $HgCl_2$.
			6 L., 5 mf. −	−0·0050	

1949	„ 27	9th egg (144 hours)	6 L., 5 mf. +	+0·0025	Ascending and descending blaze before $HgCl_2$.
	„ „	„	6 L., 5 mf. —	−0·0050	
Unrec.	„ „	„	6 L., 5 mf. +	nil ..	After injection of $HgCl_2$ sol. (N/10) abolished.
„	„ „	„	6 L., 5 mf. —	nil ..	
„	„ „	„			
„	Aug. 2..	10th egg. 12th day	6 L., 5 mf. +	— small	Bad egg. No blaze. Only polarisation.
			6 L., 5 mf. —	+ small	
			Br O + ...	— small	
			Br O —	+ small	

Second Series (1901).

„	„ 3..	1st egg (32 hours incubated)	6 L., 1 mf. +	+ small	Blastoderm size of sixpence. No vascular area.
			6 L., 1 mf. —	— small	
1976	—	„ „ „	6 L., 1 mf. +	+0·0012	
			6 L., 1 mf. —	+0·0008	Ascending and descending blaze before tetanisation.
1977	—	„ „ „	6 L., 1 mf. +	+0·0040	
			6 L., 1 mf. —	−0·0007	No further response after strong tetanisation.
Unrec.	—	2nd egg, fertile but not incubated	6 L., 5 mf. +	−0·0002 (about)	Blastoderm exposed. Only polarisation. No blaze.
			6 L., 5 mf. —	+0·0002 „	
„	—	3rd egg, do. do., and warmed to 37°	6 L., 5 mf. +	−0·0002 „	Blastoderm exposed. Only polarisation. No blaze.
			6 L., 5 mf. —	+0·0002 „	
	—	4th egg (49½ hours)	Br O + ...	+0·0030 „	Only ascending blaze. Area vasculosa distinct.
			Br O —	+0·0010 „	
„	„ 5..	5th egg (58 hours)	Br O + ...	+0·0002 „	Only ascending blaze.
			Br O —	+0·0001 „	
			Br O + ...	+0·0010 „	Normal current ascending + 0·0080 volt. Only ascending blaze. Area vasculosa distinct.
			Br O —	+0·0002 „	
„	„ „..	6th egg (59 hours)	6 L., 5 mf. ±	± nil	
			Br O + ...	+0·0012	Only ascending blaze.
			Br O —	+0·0001	
			Br O + ...	+0·0015	Only ascending blaze. Normal current was + 0·0035 (ascending).
			Br O —	+0·0002	
	„ 9..	7th egg (125 hours)	6 L., 5 mf. ±	nil (polarisn. ±)	No development whatever.
			Br O ±	nil (polarisn. ±)	No development whatever. R = 26,500 ω.
	„ „..	8th egg (90 hours)	Br O ±	nil	
		9th egg (90 hours)	Br O ±	nil (polarisn. ±)	No development whatever.
		10th egg (90 hours)	Br O ±	nil (polarisn. ±)	R through shell = 1,000,000, falling to 400,000 ω.

The symbol Br O signifies a break shock with secondary coil pushed home over primary.

"On the 'Blaze-currents' of the Crystalline Lens." By A. D. WALLER, M.D., F.R.S., assisted by A. M. WALLER. Received October 23,—Read December 4, 1902.

(From the Physiological Laboratory of the University of London, S.W.)

In the course of investigation of the effects of light and of electrical excitation on the frog's eyeball, I came to the conclusion that tissues other than retinal are coeffective in the response to strong induction hocks, and proceeded therefore to look for blaze-currents in other iving tissues.*

* 'Phil. Trans.,' B, 1901, vol. 194, p. 185.

The following extract from my note book of 1900 gives instances in which the reaction of the anterior half of the eyeball was observed to *exceed* that of the posterior half.

Frog's eyeball, entire and bisected.

Excitation by single break shock from Berne coil.

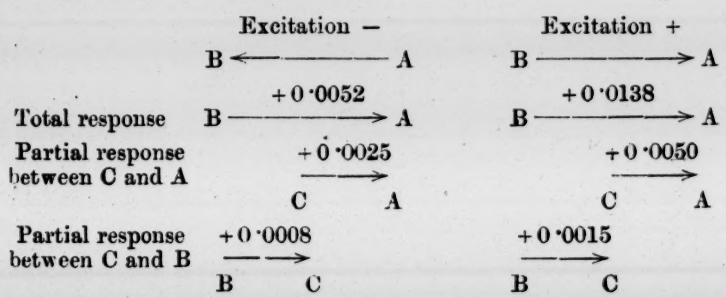
Strength of excitation.	Whole eyeball.	Posterior half.	Anterior half.
1000+	+0·0013 volt.	Nil	+0·0030 volt.
1000−	+0·0026 „	„	+0·0033 „
Another eyeball—			
5000+	+0·0017 „	+0·0002	−0·0020 „
5000−	−0·0013 „	+0·0002	−0·0040 „
	Dog's eyeball.	4 hours post mortem.	
5000+	−0·0024 volt.	Nil	−0·0030 „
5000−	−0·0008 „	„	−0·0004 „

The cornea alone gave −0·0030.
−0·0006.

The lens alone gave nothing.

In these early experiments (November 1900) no particular care was observed to avoid compressing the eyeball, and the response of the lens was therefore not obtained.

Frog's Eyeball. Tested by the ABC method. B = fundus. A = cornea. C is midway between A and B. Excitation by single break shocks of Berne coil at 1000, supplied by two Leclanché cells.



The particular point that aroused my attention in the case of the eyeball was the fact that the anterior half of the eyeball was some-

times found to give a larger response than the posterior half, and the present observations proceed from an attempt to determine the principally effective part in such reaction. And I may state at once, as my chief conclusion, that it is the crystalline lens.

The eyes upon which the determination was made, in the first instance, were those of fish—whiting and mackerel—by reason of the fact that these were for a season at my disposal quite fresh from the sea. I subsequently made similar observations on the eyes of octopus, on sheep's eyes fresh from the slaughter-house, and on the eyes of recently killed cats and rabbits; also on the eyes of an owl.

The point that was most striking in these first observations was the great endurance of the reaction in the crystalline lens as compared with its rapid disappearance from the remaining tissues of the eyeball and from the skin, and with the rapid disappearance of the direct electrical excitability of muscle. I should, as an outcome of these observations, look for the last sign of life of a fish by testing the crystalline lens, whereas in the case of man I should test a piece of skin. The reaction—as far as I have yet seen—has been completely absent from frozen fish (salmon) as received from London fishmongers. Its normal direction in the lens is "negative," *i.e.*, from external to internal pole. It is abolished by heat (70°) and by compression.

My first experiments with the eyes of fish were to ascertain on the entire eyeball what type of blaze reactions—if any—is manifested The results were as follows :—

Exp. 1. *Whiting.*—Excit. and lead-off through AB. Berne coil. Two Leclanchés in primary circuit. Single break induction shocks.

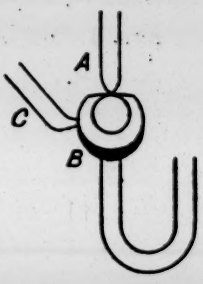


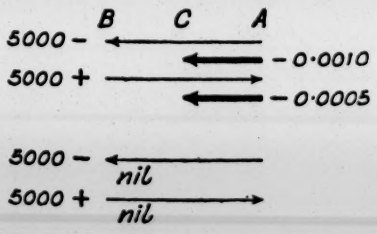
1000 +	gave	− 0·0004 volt.
1000 −	„	− 0·0007 „
5000 +	„	− 0·0010 „
5000 −	„	− 0·0017 „

Reactions after immersion in hot water.

5000 +	gave	nil.
5000 −	„	„

Exp. 2. *Whiting.*—Excit. through AB and lead off through BC or AC. (The system of notation is explained in the 'Roy. Soc. Proc.,' vol. 69, p. 183.)





Response from A to C; no response from B to C.

Lens alone.	Exc. 1000+	gives	−0·0025
	1000−	„	−0·0060
Cornea alone.	Exc. 5000+	„	nil.
	5000−	„	−0·0005
2nd lens alone.	Exc. 1000+	„	−0·0020
	1000−	„	−0·0050

Completely abolished after immersion in hot water.

A similar experiment gave similar results: in the first lens the response was completely abolished by compression, in the second lens it was greatly diminished and modified by tetanisation; the response to − + excitation being at the outset − −, then − +, then + +.



Exp. 3. *Mackerel.*—About 5 hours after death. Lens alone.

1st lens.	Exc.	1000+	gives	nil.
		1000−	„	„
		10000+	„	+0·0007
		10000−	„	+0·0005
2nd lens.	Exc.	1000+	„	+0·0050
		1000−	„	+0·0020

After strong tetanisation for 1 minute.

1000+	„	+0·0015	
1000−	„	−0·0020	

After compression there is no response at all to any strength of excitation.

N.B.—I was not alive to the orientation of the response in this experiment. The + and − signs may therefore have been transposed in these two experiments.

Exp. 4. *Mackerel.*—24 hours *post mortem*. Isolated lens.

1000+	gives	−0·0005	volt.
1000−	„	−0·0008	„
5000+	„	−0·0010	„
5000−	„	−0·0015	„

After strong tetanisation for 1 minute.

5000+	„	+0·0010	volt.
5000−	„	−0·0008	„

After compression no further response.

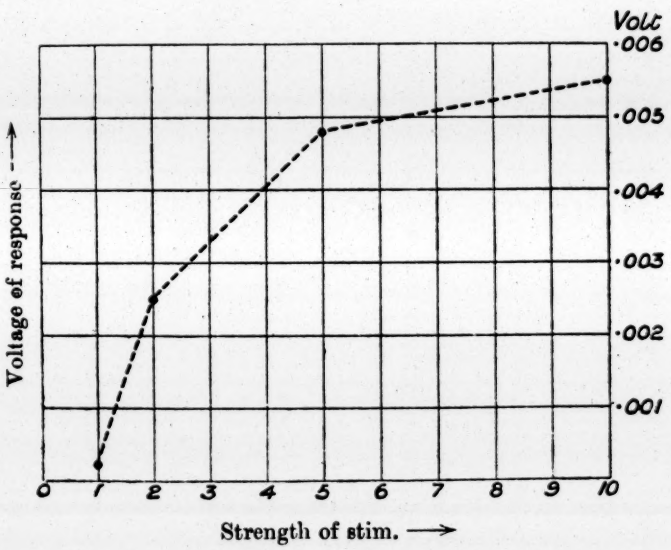
There was no appreciable alteration of resistance after tetanisation.

Exp. 5. *Whiting.*—4 hours *post mortem.* Isolated lens.

1000 + gives +0·0002
1000 − „ −0·0010

500 + gives	nil	nil
1000 + „	+0·0002	+0·0002
2000 + „	+0·0025	+0·0020
5000 + „	+0·0050	+0·0040
10000 + „	+0·0055	+0·0050

No further response after compression.



Exp. 6. *Mackerel.*—48 hours *post mortem.* Isolated lens.

Exc.	1000 +	gives	−0·0003 volt.
	1000 −	„	−0·0004 „
	10000 +	„	−0·0010 „
	10000 −	„	−0·0015 „

The response is abolished by plunging lens in hot water.

Exp. 7. *Octopus.*—15 hours after removal from water.

The *isolated eye* gives no distinct response either to light or to electrical excitation.

Its isolated lens gives to

1000 +	a response of	+0·0040 volt.
1000 −	„	+0·0005 „
5000 +	„	+0·0015 „
5000 −	„	+0·0010 „

Later and with altered position of lens on electrodes.

1000 +	a response of	− trace.
1000 −	„	− 0·0004 volt.
5000 +	„	− 0·0005 „
5000 −	„	− 0·0010 „

Exp. 8. *Dogfish.*—Isolated lens.

1000 +	gives	− 0·0012 volt.
1000 −	„	− 0·0020 „

The response is abolished by pressure.

Exp. 9. *Cuckoo Fish* (white).—Some hours (? 4 or 5) after death. The eyeball gives no response to light or to electrical excitation.

Its lens, to	5000 +	gives	− 0·0020 volt.
	5000	„	− 0·0050 „

Set up in connection with three electrodes so as to be excited through AB, and led off through AC or BC (as for the entire eyeball). The lens responses are as follows:—

	B	C	A
AB Exc. 5000 −	←	——	——
AC Resp. − ·0035		←	—
AB Exc. 5000 +	——	——	→
AC Resp. − ·0025		←	——
AB Exc. 5000 −	←	——	——
BC Resp. − ·0015	←	—	
AB Exc. 5000 +	——	——	→
BC Resp. − ·0006	←	—	

Exp. 10. *Cuckoo Fish* (yellow).—Isolated lens. A few hours (? 4 or 5) after death.

Exc. 1000 +	gives	− 0·0030 volt.
1000 +	„	− 0·0050 „

On returning to London, I first tried salmon's eyes, and then adopted the cod's eye as affording a constant supply of suitable material.

Exp. 11. *Salmon* (from a London shop).—Isolated lens. No response. The fish had been kept in ice.

1000 +	gives nil.		
1000 −	„	„	−
10000 +	„	„	
10000 −	„	„	−

Exp. 12. *Codfish* (1st).—Isolated lens. Fish said to have been brought to shore on the previous day.

5000 –	gives	– 0·0040, – 0·0020
5000 +	„	– 0·0010, – 0·0010

Photo. 4242 is now taken—

5000 +	„	– 0·0009
5000 –	„	– 0·0022

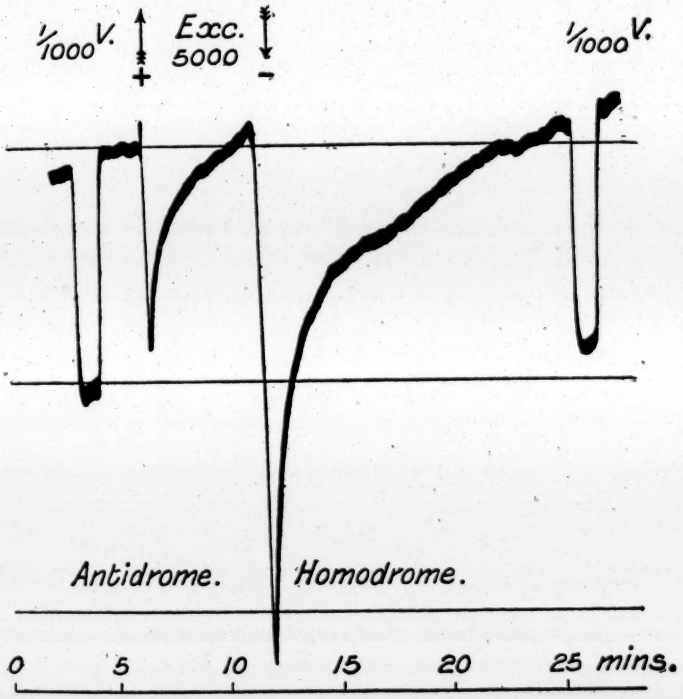


Photo. 4242.—Codfish Lens. Anti- and homodrome responses.

The normal current was – 0·0008.
The other lens gave no distinct response.
The same lens next day gave

to 5000 +	– 0·0005
–	– 0·0015

abolished by heat.

Exp. 13. *Codfish* (2nd).—Lens. 1 or (?) 2 days after capture of the fish, tested by the ABC method for determination of the seat of the response. (Given *in extenso.*)

Exp. 14. *Codfish* (3rd).—Lens. ? second day after capture.

5000 +	– 0·0007
5000 –	– 0·0033

10000 +	− 0·0013
10000 −	− 0·0048
10000 −	− 0·0052
10000 +	− 0·0015
5000 −	− 0·0027
5000 +	− 0·0008

Moderate compression gives − deflection off scale in consequence of mechanical excitation. Considerable compression abolishes all response.

The other lens gave no response; the eye from which it had been removed was evidently injured, being full of blood.

Exp. 15. *Codfish* (4th).—Supplied as fresh; neither lens gave any response.

Exp. 16. *Mackerel.*—Lens. Fish reputed fresh.

5000 +	− 0·0004
5000 −	− 0·0011
Moderate compression	− ——
5000 +	+ 0·0002
5000 −	+ 0·00015
Severe compression	− ——
5000 +	nil.
5000 −	,,

Exp. 17.—The lenses of four sheep's eyes brought from the slaughter-house and tested within 3 hours after death gave no distinct response.

Exp. 18. *Sheep's Head.*—1st lens removed from the eye 4 hours *post mortem* with least possible compression. Normal current = + 0·003 decreasing.

	1000 +	nil.
	−	,,
	5000 +	− 0·0008
	−	− 0·0030
	+	− 0·0002
	−	− 0·0020
Same lens	+	− 0·0004
Next day	−	− 0·0007
	+	− 0·0002
	−	− 0·0003

No response after immersion in hot water.

2nd lens removed from the eye 18 hours *post mortem.*

5000 +	− 0·0005
−	− 0·0012

Then photo. 4248—

	5000 +	− 0·0010
	„ −	− 0·0014
	„ +	− 0·0005
	„ −	− 0·0008

Exp. 19. *Cat.*—Lens. 1½ hour *post mortem*.

1st lens	5000 +	− 0·0003
	„ −	− 0·0008

Response abolished by compression.

2nd lens	5000 +	− 0·0002
	„ −	− 0·0006
Same lens next morning	+	− trace
	−	+ trace

Exp. 20. *Mackerel.*—Lens. Reputed fresh.

	1000 +	+ 0·0002
	−	− 0·0001
	5000 +	+ 0·0003
	−	− 0·0004
	10000 +	+ 0·0004
	−	− 0·0006

Responses homodrome throughout.

Exp. 21. *5th Codfish.*—Lens.

1st lens	5000 +	− 0·0002	
	−	+ 0·0001,	− 0·0002
	10000 −	− 0·0006	
	+	− 0·0002,	+ 0·0003
2nd lens	10000 +	− 0·0001	
	−	+ 0·0001	

Attempts were made to test the last two lenses by lateral eye rotation; the results were uncertain and variable. Similar trials in other cases were equally variable.

Exp. 22. *Cat.*—Lens. 5 hours *post mortem*.

1st lens.	Initial current	+ 0·0011	
	1000 + and −	gave nil, nil.	
	5000 + „ −	„	− 0·0004 and nil.
2nd lens	5000 +	„	− 0·0015
	5000 −	„	− > 0·0030 (off scale).

Exp. 23. *Brill.*—Lens (? 24 hours).

Initial current		− 0·0036
Exc. by single break	1000 +	− 0·0020

	1000 −		− 0·0080
	100 +		− 0·0003
	100 −		− 0·0015
After compression	1000 +	and −	nil, nil.
Electrodes tested by	1000 +	„ −	„ „

Note.—The lens of this fish is rather smaller than convenient.

Exp. 24. *Frog.*—R. temporaria.

Entire eyeball. Initial current		=	− 0·0030
Exc. by single break	100 +	=	+ off (>0·002)
„ „	100 −	=	+ off (>0·002)
Its isolated lens.	100 + and −	=	nil, nil.
	1000 +	=	− 0·0005
	1000 −	=	− 0·0010

Subsequently both responses were observed to be homodrome, viz., + to + exc., and − to − exc.

The isolated lens of the other eyeball gave similar results.

The lens of the frog's eye is inconveniently small, nevertheless, with due care, typical effects can be observed upon it, viz., negative responses* to both directions of excitation, the homodrome exceeding the antidrome response. The normal and typical response of the entire eyeball was, as previously stated, positive to both directions of excitation. Rana temporaria has, in my experience, given clearer effects than Rana esculenta.

Exp. 25. *Cat.*—5 hours *post mortem*.

1st lens. Initial current		=	− 0·0030
Exc. by single break	5000 +	=	− 0·0005
	5000 −	=	− off scale (>0·0030)

After compression the positive current was nearly doubled, and there was no response to 5000 + 5000 −.

The 2nd lens, less carefully removed, gave no response to 5000 + and −.

Of five successive cod's heads supplied to me in London as fresh, all but one gave responses of typical character, as illustrated by the photogram; in every case, however, the lenses of the two eyes were

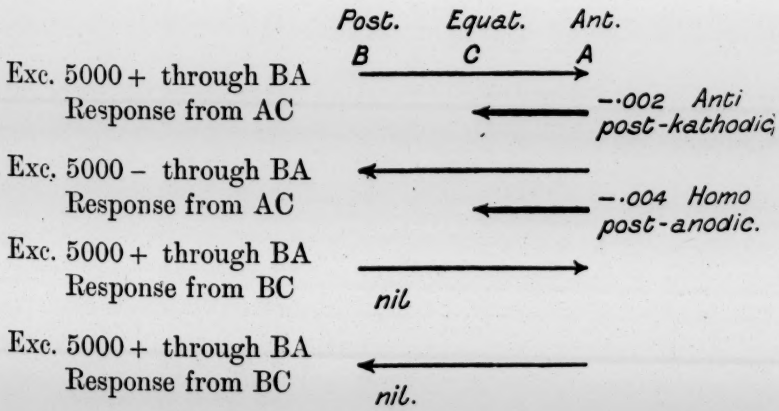
* Throughout this paper, *positive* current signifies current through the (eyeball or) lens directed from posterior to anterior surface, and *negative* current the reverse of this. In one experiment (Exp. 13, with reversed zincs) the direction of the readings unavoidably breaks this conventional rule. And, indeed, in other experiments this rule has occasionally been broken, as in Exp. 13, in order to set aside conceivable fallacies of the electrodes, kept in an invariable relation to each other—*e.g.*, an invariable inequality between them, or a gravitation current of liquid from A to B, or a constant difference of area, and therefore of current-density at A and B.

unequally good; in three instances one of the lenses gave no response, and in one of these three instances the eyeball was filled with blood. I think the difference between the two eyes must have been due to the fish having been killed by stunning, or it may be that in transit to London one of the eyes had suffered compression. But whatever the real cause of the difference may have been, the lenses of fish obtained in London were far less satisfactory than those of fish directly taken from the sea. In the latter case, both lenses, if carefully removed, were equally effective (provided the fish had not been stunned in the usual way on removal from the hook).

Similar effects are obtainable on the crystalline lens of the mammalian eye; but it is essential to avoid any undue compression of the globe. Thus I completely failed to observe any effect on the lens of eyes removed from the orbit of dogs and cats in the usual manner, also on the lens of sheep's eyeballs brought fresh from the slaughter-house.

But with lenses carefully removed from the eyes of a fresh sheep's head and of a recently killed cat, typical and regular responses were obtained, which were abolished by intentional compression as well as by immersion in hot water (60° to 70°).

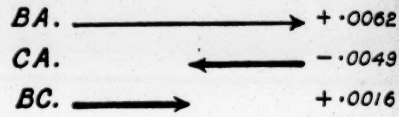
I think it desirable to give *in extenso* one experiment (No. 13) to illustrate the precise nature of experimental evidence and the system on which it is taken down. It is very easy to make sure of the direction of a current used for excitation in relation to a total or bipolar response, but it is not easy without a strictly systematic plan to make sure of this relation when a partial or unipolar response is under investigation. It is advisable for the latter purpose to carefully verify the connections of the ABC key* so that directions of deflection may immediately signify directions of current between the points of investigation, and be noted accordingly in a legible form that can be readily reviewed.



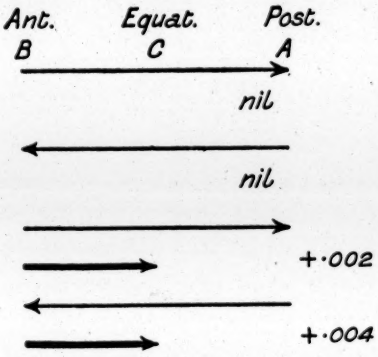
* Described in 'Roy. Soc. Proc.,' vol. 69, p. 181.

I give the normal currents, not that they are essential, but because an expert reader might wish to know them.

Normal, *i.e.*, accidental current

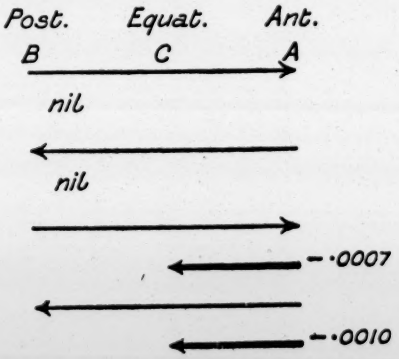


All response was from the anterior pole; none from the posterior. The series was repeated with similar results, and now the zincs of the electrodes were transposed so that the connections were—

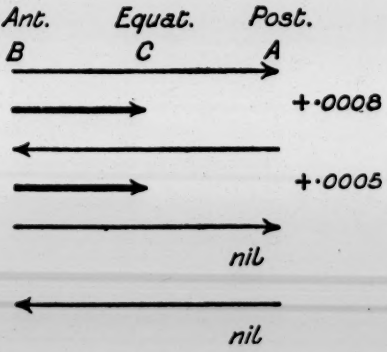


i.e., as before response only from anterior and not from posterior pole.

Now the lens is turned round—the zincs left reversed as before—and the connections thus revert to :—



Finally, the zincs are replaced so that we have the lens left reversed—



The lens is submitted to compression, after which there is no response of any kind, either total or partial; it is placed for a few minutes in hot water until coagulated white, and again tested without any response. The temperature at which the first obvious sign of coagulation was observed was 38°. The lens was completely white at 48°. Beyond 50° no further increased whiteness could be seen.

I conclude from this and similar experiments—

1. That a crystalline lens of suitable size is a good object upon which to study the nature of blaze-currents.
2. That a "blaze-current" is a physical sign of the "living" state.
3. That a blaze-current may be post-kathodic as well as post-anodic, antidrome as well as homodrome.
4. That the direction of blaze-currents in the lens is negative or ingoing, *i.e.*, from external or anterior to internal or posterior pole.

ADDED DECEMBER 4, AFTER THE RECEIPT OF DR. DURIG'S PAPER.

(See p. 212.)

Exp. 25. Nov. 1. *Owl.*—Positive responses of the anterior half of the eyeball; negative responses of the lens alone; positive responses of the cornea alone.

1st eyeball. *Anterior half.* 7 minutes *post mortem.*

Initial current		=	−0·0008 to −0·0018 volt.
Exc. by single break	1000 +		+0·0005
,, ,,	−		+0·0005
,, ,,	5000 +		+0·0020
,, ,,	−		+0·0015
,, ,,	−		+0·0015
,, ,,	+		+0·0015

Lens alone.

	10000 +	−0·0005
	−	−0·0020

Cornea alone.

Single break	10000 +	nil.	
,,	10000 −	nil.	
Several	10000 +	+0·0012,	+0·0010
,,	10000 −	+0·0020,	+0·0020
Tetanisation	10000 +	+0·0015	
,,	10000 −	+0·0025	

Cornea plunged in hot water.

Tetanisation	10000 −	nil.
,,	10000 +	+0·0005

2nd eyeball. *Anterior half.* 2 hours 10 minutes *post mortem.*

Initial current	=	+0·0005 to nil.
Exc. by single break	5000 +	+0·0010 volt.
,, ,,	−	+0·0015 ,,

Lens alone.

Exc. by single break	5000 +	− >0·0030 (off scale)
,, ,,	−	− >0·0030 ,,
,, ,,	1000 +	− 0·0004
,, ,,	−	− 0·0010

Cornea alone.

Single break	5000 +	nil.
,,	−	+0·0007
,,	10000 +	nil.
,,	−	+0·0009
Several	10000 +	nil.
,,	−	+0·0010

Lens replaced.

Single break	1000 +	−0·0007
,,	−	−0·0012
,,	5000 +	+0·0003
,,	−	+0·0005

Exp. 26. *Codfish.*—Removed from water on Friday, November 7, at 4 P.M.

1st lens. (November 8, 4.45 P.M. Demonstration at Physiological Society.)

			Response.
Exc. by single break	1000 +		− 0·004 volt.
induction shock of Berne coil with 2 Leclanchés in primary circuit	1000 −		− >0·005 (off scale)
	1000 +	after compression	nil.
	1000 −		nil.

2nd day. 1st lens (Nov. 9)

	10000 +	−0·0013	antidrome blaze
	−	−0·0010	homodrome ,,

2nd lens (Nov. 9)

6 P.M.	500 +	−0·0002	antidrome ,,
,,	−	−0·0018	homodrome ,,
	600 +	−0·0003	antidrome ,,
,,	−	−0·0018	homodrome ,,
10 P.M.	,, +	−0·0003	antidrome ,,
,,	−	−0·0017	homodrome ,,

	700 +	− 0·0013	antidrome	blaze
	−	> − 0·0028	homodrome	„
	600 +	− 0·0008	antidrome	„
	„ −	> − 0·0028	homodrome	„
	„ +	− 0·0006	antidrome	„
	„ −	− 0·0020	homodrome	„
	+	− 0·0004	antidrome	„
	−	− 0·0018	homodrome	„
	+	− 0·0003	antidrome	„
	−	− 0·0011	homodrome	„
	½ hour interval.			
	600 −	− 0·0031	homodrome	„
3rd day. 2nd lens (Nov. 10)				
	600 −	> − 0·0038	homodrome	„
	500 +	− 0·0011	antidrome	„
	„ −	− 0·0038	homodrome	„
	400 +	− 0·0003	antidrome	„
	„ −	− 0·0016	homodrome	„
4th day. 2nd lens (Nov. 11)				
	400 +	− 0·0008	antidrome	„
	−	− 0·0006	homodrome	„
	500 +	− 0·0005	antidrome	„
	−	− 0·0005	homodrome	„
	400 +	− 0·0001	antidrome	„
	−	− 0·0002	homodrome	„
5th day. 2nd lens (Nov. 12)				
	500 +	nil.		
	−	nil.		
	1000 +	nil.		
	„ −	nil.		
	5000 +	− 0·0001	polarisation	
	„ −	+ 0·0002		
Tetanise	10000 +	+ 0·0010	blaze ?	
	„ −	− 0·0010		
6th day. 2nd lens (Nov. 13)				
Tetanise, 12 P.M.	10000 +	− 0·0005	blaze ?	
	„ −	− 0·0006		
7th day. „ 2 P.M.	10000 +	+ 0·0007	polarisation	
	−	− 0·0008		
Sing. shock	10000 +	− 0·0001	polarisation	
	−	+ 0·0002		

This is the longest period during which I have followed the response of an isolated lens.

Indubitable response, negative to positive (antidrome) and negative to negative (homodrome), to single shocks, was observed on the 4th day, absent on the 5th day.

Response, negative to negative, with strong tetanisation in both pairs of directions, was still present on the 5th and 6th days. But on the seventh day the only visible effects were of polarisation direction, *i.e.*, the lens was judged to be dead.

Exp. 27. (November 19, with Sir J. Burdon-Sanderson.)—Lens of rabbit eye, removed 4 hours *post mortem*. Normal current 0·0150 volt positive declining.

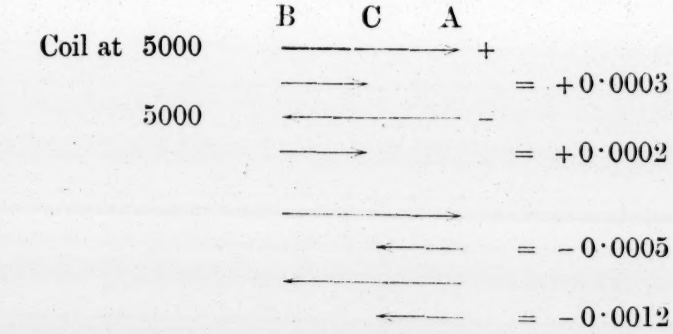
1*st lens.*

Excitation by single shock,

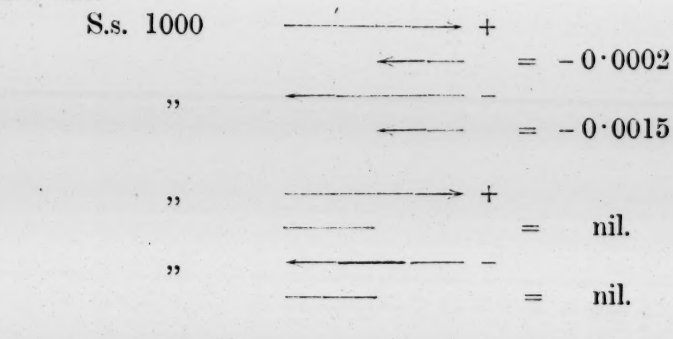
Coil at 1000 + = −0·0006
− = −0·0010 followed by positive after-effect.

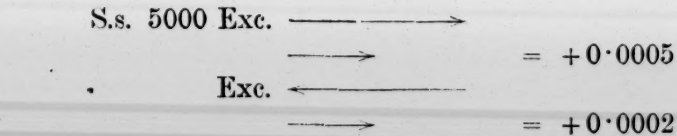
Excitation with the ABC method.
By single induction shocks.

(1st day).



2*nd lens.*





Exc. ——————→
←—— = −0·0008
Exc. ←————
←—— = −0·0013

S.s. 5000 Exc. ——————→ +
Resp. ←———— = −0·0004

Exc. ←———— −
Resp. ←———— = −0·0008

Next day. 1st lens.
(2nd day.)
S.s. 5000 Exc. ——————→ +
Resp. ←———— = −0·0021

Exc. ←———— −
Resp. ←—— = −0·0006

Exc. ——————→ +
Resp. ←———— = −0·0016

Exc. ←———— −
Resp. ←———— = −0·0005

Same lens compressed.
S.s. 5000 Exc. ——————→ +
Resp. ←———— = −0·0003

Exc. ←———— −
Resp. ———— − trace.

Same lens coagulated by
heat (53° C.—62° C.)
Exc. ——————→ +
Resp. ——— nil.
Exc. ←———— −
Resp. ——— nil.

Tetan. Exc. ——————→ +
Resp. ———— nil.
Exc. ←———— −
Resp. ———— nil.

2nd lens.
S.s. 5000 Exc. ——————→ +
Resp. ←———— = −0·0008

Exc. ←———— −
Resp. ←———— = −0·0007

Tetan. Exc. ——————→ +
Resp. ←—————— = −0·0030

Tetan. Exc. ←—————— −
Resp. ←—————— = −0·0018

The lenses were now heat-coagulated in normal saline; opalescence appeared at 52° to 55°; coagulation became complete at 62° to 72°. No trace of blaze-current could be obtained from either of the two leases after the heat-coagulation.

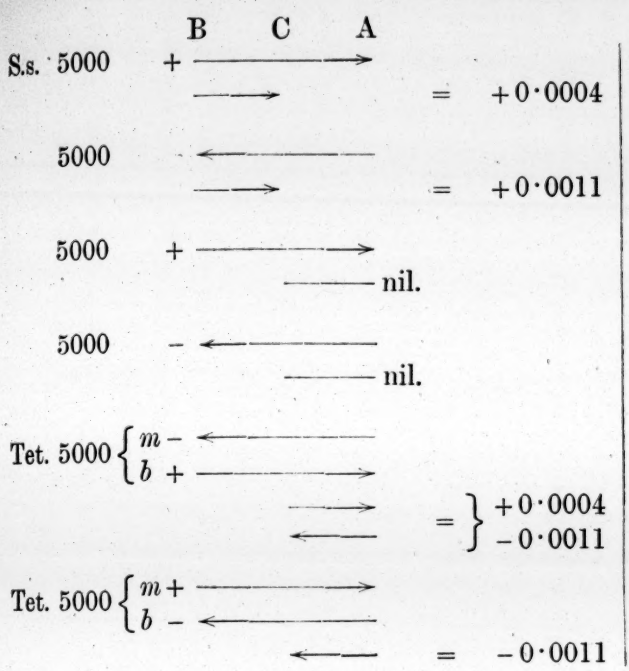
Exp. 28. *Lens of Rabbit's Eye.*—24 hours *post mortem.*
2nd day.
Normal current −0·0036.

1st lens.		*Excitation.*		*Response.*
(2nd day).	S.s. coil at	1000 −	=	−0·0021
		+	=	−0·0003
(3rd day.)		10000 −	=	−0·0003
		+	=	a trace −
		5000		
2nd lens. (2nd day.)				
Normal current −0·0019	S.s.	1000 −	=	−0·0031
		+	=	−0·0015
(3rd day.)		5000 −	=	−0·0017
N.C. = 0		+	=	−0·0002
		1000 +	=	nil.
		−	=	a trace −

Exp. 29.—*Lens of Rabbit's Eye.*
N.C. = +0·0025.

	S.s.	5000 +	=	−0·0040
		−	=	−0·0060
Strong tetanisation in both pairs of directions		+	=	−0·0002
Neg. deflection of −0·02		−	=	+0·0003

Exp. 29. *Pigeons' Eyes.*—25.11.02.
The unpolarisable electrodes A and B are applied to the intact corneæ of the eyes of a decapitated pigeon.
Excitation is led in by A and B, the response is led out by AC or by BC (previously compensated).



The first pair of excitations is effective. B is in each case the seat of an ingoing response.

The second pair of excitations is ineffective. A has presumably been exhausted by previous excitation.

Response of A is, however, elicited by tetanisation in both pairs of directions.

Exp. 30. *Rabbit's Eyes in situ.*—A similar series of trials gave in each case a small outgoing effect at each eye, presumably due to corneal response.

In the discussion, attention was drawn to the details of certain experiments—viz., Exps. 3, 7, and 20—as being in disagreement with the general rule that the lens response is negative. Exps. No. 3 and 7 were made before I had learned the importance of attending to the orientation of the lens. Exp. 20 (and Exp. 5) are instances of what, in my experience, have presented themselves as transitional types intermediate between the typical physiological response and the ordinary polarisation effects of exhausted or dead organs. I have thought it possible that certain irregularities of response occasionally met with might have been due to unavoidable injury of the *Retractor Lentis* (Campanula Halleri), described and figured by Beer.*

After Exp. No. 7, I was always careful to mark the external or corneal pole of the lens *in situ* by a speck of moist china clay, with which the clay end of electrode A was subsequently brought into contact; the opposite pole rested on electrode B.

I may take this opportunity of stating that the eyes of crabs and of lobsters gave ingoing blaze-currents (from A to B) to both directions of excitation.

* 'Pflüger's Archiv,' vol. 58, p. 574.

"A Contribution to the Question of Blaze Currents." By Dr ARNOLD DURIG, of Vienna. Communicated by AUGUSTUS D. WALLER, M.D., F.R.S. Received November 20,—Read December 4, 1902.

(From the Oxford Physiological Laboratory.)

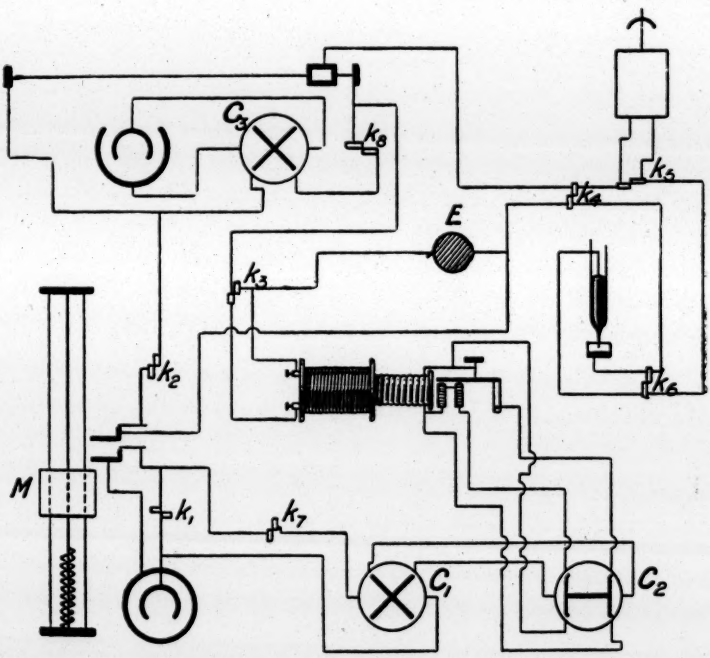
(Translated from the German by Miss F. Buchanan, D.Sc.)

The numerous experiments of Waller on this subject have raised the question as to whether the phenomena of the so-called "blaze currents" are to be regarded as a proof of the persistence of life. It is the purpose of the present communication to give the results of a few experiments which bear upon the subject. These relate principally to the response of the eyeball, but also to that of a few other organs of the frog, to single induction shocks. As they are not yet definitively concluded, I propose here to give only a short description of them, and not to refer to their theoretical significance beyond pointing out that the results obtained must be taken into account before deciding whether or not the currents in question furnish an unmistakable sign of vitality in tissues.

As regards method, the great importance of determining the exact moment after excitation, when the first trace of a current appears, must be insisted upon. It is essential to know whether or not it is after a latent period, and to know its direction, in order to ascertain whether, in addition to the reaction of living tissue, polarisation effects are also being observed. The capillary electrometer has advantages over the more inert galvanometer for this purpose, since the velocity with which the meniscus moves when it first begins to be acted upon by a difference of potential, as determined from the photographic record of the excursion, would enable one to recognise the existence of a polarisation current when it is in the same direction as the "blaze current." Such records have, however, still to be made. The diagram shows the arrangement of the apparatus employed.

By the commutator C_1 in the primary circuit, the direction of the exciting current could be reversed. By means of C_2 the current could be sent to the primary coil either direct or after passing through a Neef's hammer. The key, K_1, served to determine whether the current should be made or broken, the spring-myograph, M, opening accordingly, either the primary circuit itself, or a bridge short-circuiting it. It was thus possible to excite the preparation in immediate succession by ascending or descending, make or break, induction shocks, and to excite it after faradisation by a single shock in either direction. Another contact, for short-circuiting the galvanoscope, was so arranged as to be broken by the myograph immediately after

it had broken, or made, the primary circuit. The distance between this contact and one in the primary circuit was of course so chosen that the induction current itself should produce no effect in the measuring instrument. As the sudden letting in of a resting current would have caused great disturbances in the measuring instrument when the short-circuit was broken, it was necessary to compensate this very carefully beforehand. By means of the key K_2 the compensation could be tested immediately before letting go the myograph. The myograph-stand itself was placed in an adjoining room in order that the movement of its steel rod might not affect the galvanometer. It could however be set in motion from the table on



which the rest of the apparatus was placed. The secondary coil could be short-circuited by the key K_3; the measuring instrument by the key K_4, the keys K_5 and K_6 allowing of the introduction of either galvanometer or capillary electrometer to serve as such, in order that the results obtained with the one instrument might be quickly checked by the other. Although the arrangement was well adapted for concordant observations, it was defective, not only in the absence of the apparatus for recording the movements of the electrometer, but also in another more important respect. The necessity of distinguishing what is due to polarisation in the study of "blaze currents" has already been pointed out. With this in view observations ought to be made in such a way as to eliminate the effects of polarisation as much as possible, as would be accomplished by using, to produce a single exci-

tation, two almost instantaneous induction shocks, as nearly as possible equal in strength, but opposite in direction, according to the method first introduced by Bernstein and by Hermann in rheotome experiments. The use of an instantaneous make and break contact for the myograph and the introduction of a coil, suitably wound so as to avoid induction, or of an incandescent lamp, into the primary circuit, would have been a simple means of attaining this end.

To fully acquaint the reader with what is new in the following results, and with what is merely confirmatory of the observations of others, that is to say of those of Waller, would make the present communication too lengthy. I will, therefore, take it for granted that Waller's researches, which form the starting-point of the subject, are known.

For the experiments on the eye the organ was kept either in daylight or in the dark. The whole eyeball, or the special part of it that was being investigated, lay in a slight depression of a kaolin (unpolarisable) electrode, and was connected by a thread of wick moistened in physiological salt solution to the second electrode. The experiments were always made on the freshly-removed eye, and so relate to Waller's first stage only. A few observations were made on curarised frogs, the optic nerve being exposed by the removal of part of the skull, and looped round by a piece of wick to lead it off to the galvanometer. Since a certain amount of bleeding ensued when the optic nerve was cut, it was especially ascertained at the end of each such experiment that the eye-circulation was maintained. In all the experiments attached pieces of muscle and connective tissue were carefully removed from the eyeball.

The results obtained by the two measuring instruments, the galvanometer and the capillary electrometer, are fully in accordance with one another; the responses to single induction shocks appear, however, distinctly smaller in the galvanometer than in the capillary electrometer. In the latter instrument the effect exceeds the current of rest in size, being more than twice and sometimes several times as great when cornea and optic nerve are led off. Since the response to light stimulation is hardly perceptible in the capillary electrometer, one sees at once how considerable are the differences of potential of which the "blaze currents" are the manifestation (Waller).

Experiments on the whole Eyeball when the middle of the Cornea and the cut end of the Optic Nerve are led off from.

The cornea is always positive to the optic nerve. If the electrode is moved away from the middle of the cornea nearer to the equator of the eyeball, the current of rest becomes less, and if moved beyond the equator to the posterior part of the eyeball it may be reversed.

It is possible to so place the electrode that there shall be no resting current, and this is especially the case in the curarised animal in which the eye-circulation is maintained (Hermann, Holmgren). When the experiment lasted some time the current of rest became reversed, even when the middle of the cornea was led off from. The *response to light* is always in the positive direction, *i.e.*, in the same direction as the current of rest. This positive variation is preceded by a negative initial jerk (Vorschlag). When the light is extinguished a further positive change occurs which subsequently diminishes, at first quickly and then more slowly. Within certain limits the extent of the excursion increases with the duration and strength of the illumination. No negative initial jerk could be observed in the eye of the living frog in response to light; the increase which occurred when the illumination ceased was, however, greater and lasted for a longer time than in the excised eyeball. The return of the needle took place no more quickly than in the excised eye, notwithstanding the maintenance of the circulation and the consequent greater activity of assimilation processes.

Excitation of the eye in the dark by light after previous excitation by electric currents usually resulted in an augmentation of the excursion, the excursions actually observed being greater than the sum of the after-effect produced in response to the induction current and the excursion which is the normal response to illumination alone. A still more striking augmentation of the effect occurred after previous faradisation (Waller). The fact that the augmentation occasionally eluded observation may be due to the circumstance that the sensitiveness of the galvanometer had often to be reduced by a tenth on account of the largeness of the excursion due to the combined effect of "blaze-currents" and light currents.

Electrical effects in response to light are given not only by the *pars optica retinæ*, but can be obtained also from the anterior part of the eyeball alone, the excursions, which could only be observed by galvanometer, being, however, very small. The possibility that the heat of the small incandescent lamp which was used for giving the light-stimulus may have been responsible for the effect must be borne in mind, and alterations which may have been produced in the muscles of the iris must also be taken into account. I am at present engaged in making the further experiments that are required with regard to this matter.

The results obtained with the eyeball in response to *excitation by electrical currents* are not in any way affected by the absence or presence of light. The currents which occur in the uninjured eye in response to such stimulation are, therefore, independent of the effects produced by light, and are to be sharply distinguished from them. (Waller.) This is confirmed by the following new observation:—

When the one leading-off electrode is moved away from the cornea towards the equator of the eyeball, not only is the difference of potential between the two poles diminished, but also the effect produced by an induction shock, which was before considerable, has now almost vanished. Instead of an excursion extending over 600 divisions it now extends over only 20 or 30, and its direction occasionally varies with that of the induction current. When the electrode is replaced on the cornea the original large deviation is again produced in response to stimulation.

When the breaking of the second contact by the myograph lets the preparation into the capillary electrometer, two different main types of response may be distinguished by the form of the observed effect. The curve is either distinctly diphasic, the first phase being negative, *i.e.*, in the opposite direction to the resting current, and the second, which is always considerably stronger, being positive and lasting for a much longer time; or a purely monophasic variation is obtained which is always positive. The negative part of the diphasic variation was so soon over that when, by means of a turn-over key, the preparation was connected with the galvanometer by hand instead of by the myograph, the most that could be observed as the last trace of the first phase was, on account of the relatively great inertia of the galvanometer, an excursion of a few divisions only, by reason of the relatively great inertia of the galvanometer. The phenomenon of the diphasic effect occurs when the induction currents—especially break-shocks—are in the same direction as the "blaze current" is expected to be in, which suggests that one is here dealing with pure polarisation phenomena which do not show themselves when the exciting current is in the opposite direction, as the two effects are then in the same direction. Experiments with this in view are needed to make the matter clear. No explanation has, so far as I am aware, been offered before of the occasional absence of the first phase, which, if it is merely a polarisation effect, must be a physical change independent of any processes in living tissue.

Faradisation produces a distinct augmentation of the resting current, which, however, varies greatly in size and duration in different preparations. Break and make induction shocks in either direction are effectual after faradisation, and produce positive variations which appear as an increase of the augmentation. It is only after very strong faradisation that single induction shocks are ineffectual; in this case the initial augmentation of the pre-existing current (due to the faradisation) soon begins to disappear, the diminution continuing until a resting current in the opposite direction is produced.

The fact that the effects of excitation are so small when the equator

and the posterior part of the eyeball are led off from, is sufficient to suggest that the real seat of origin of the "blaze currents" is to be sought for in the anterior part of the eye. Waller, who at first* regarded the "blaze currents" as having their origin in the retina, has in a subsequent† publication, stated that tissues, other than retinal, take part in their production.

Observations on Separate Parts of the Eyeball.

If the *posterior part of the eye* alone is placed on the electrodes and stimulated, the response is the same as in the whole eye when the equator is led off from, *i.e.*, only a very slight variation occurs. If the *anterior part of the eye* is investigated by itself, the effects produced are the same as when cornea and optic nerve are led off from. It was, therefore, obvious that cornea, lens, and ciliary body must next be investigated separately. One would especially expect to get electromotive effects with the *ciliary ring* on account of the attached iris and its muscles. Experiment showed that break and make induction shocks in either direction always gave deviations and excursions in a definite direction which cannot, however, be said to be positive in relation to the resting current on account of the inconstancy of the latter, which is due, no doubt, to the irregular construction of the organ. The preparation is, moreover, very perishable, and very soon gives no effect at all, or only (polarisation ?) effects, the direction of which varies with that of the exciting current.

The *lens* is much more easy to work with. From it one obtains, according to the position of the electrodes, well-marked though weak resting currents in a constant and definite direction. In response to excitation, effects in one direction (the positive) occur whatever the direction of the exciting current. They are frequently preceded by negative initial jerks when the exciting current is in the same direction, just as is the case in the whole eye. Although the "blaze currents" are in this case much stronger than they are in the ciliary body, those of the two together are not nearly sufficient to account for the currents in the whole eyeball. The only remaining part of the eye in which to seek for the principal seat of origin of those large differences of potential, which occur in the response of the whole eye, was now *per exclusionem* the *cornea*, and, according to expectation, this was found by experiment to give strong electromotive effects. The preparation was made by cutting into the eyeball at the edge of the cornea with scissors, and then cutting right round the limbus. The

* Waller, "On the Retinal Currents of the Frog's Eye, excited by Light and excited Electrically," 'Phil. Trans.,' B, vol. 193, p. 123.

† "On the 'Blaze Currents' of the Frog's Eyeball," 'Phil. Trans.,' B, vol. 194, p. 183.

inner surface of the removed cornea was placed on a little projection of the kaolin of the one electrode, the outer surface was connected by a thick piece of wick with the other. In response to every stimulation, large "blaze currents" occurred, which in all respects resembled those of the whole eyeball, even in their intensity, so that one would not have known that the experiment was not being made with the whole eye unless one had seen the preparation. The resting current was from the inner to the outer surface; each induction shock, in whichever direction, produced an effect in the same direction as the resting current. When the excitation was also in the same direction as the resting current, this (outgoing, Transl.) response was preceded by a well-marked initial effect in the opposite direction.

According to these observations, the principal place in which the "blaze currents" of the eyeball originate appears to be the cornea, while the ciliary ring and lens contribute thereto. The retina plays quite a subsidiary part in their production. The results of these experiments on the cornea give support to the analogies between skin and cornea in their behaviour to excitation, to which Waller, by comparing the eyeball and skin currents, has already drawn attention, although it must not be forgotten that, owing to the difficulty in preparing it, the cornea is a much damaged tissue.

The fact that the cornea, an epithelium which is free from mucous cells, possesses strong electromotive properties, has a distinct bearing on one question which has been sometimes discussed, namely, that as to whether it is the mucous cells or the ordinary epithelial cells that are the cause of the occurrence of differences of potential in skin, tongue, &c.

There is another point of interest in connection with these experiments which may be here mentioned, which is, that the "blaze currents," after reaching their maximum, seldom diminish at a constant rate. The diminution frequently takes place in a series of steps, *i.e.*, the current, after beginning to diminish at a regular rate, suddenly ceases to diminish for a few seconds or is even slightly augmented, thus causing a projection on the curve; it then diminishes again at its previous rate, being interrupted at intervals by fresh augmentations. This phenomenon also requires further study. In order not to increase the length of this communication, the curves obtained and the numbers from which they were derived will not be introduced.

It is the retina especially from which one might have expected to get large electromotive effects. The fact that the response to excitation observed in it was so small (and the little there was may not even have been entirely due to the retina, since the optic nerve and the sclerotic remained attached) is enough to suggest the question whether "blaze currents" may really be taken as a sign of the persistence of life,

their absence as a sign of its cessation. For the solution of this question, a few experiments were undertaken with other organs of the frog, namely, liver, kidney, and ovary of freshly-killed animals. With such specifically-surviving organs as liver and kidney one would have especially expected to find the currents which are supposed to be a sign of life.

Experiments on Liver, Ovary and Kidney.

Only one out of all the experiments made gave a result which could in any way be compared with that obtained from the eye. The other preparations all gave electromotive effects which were often very considerable, but which cannot be otherwise regarded than as polarisation effects, since they varied with the direction of the exciting current and were always opposed to it. The one experiment which formed the exception can hardly be said to agree with the observations on the eye, since, although the excursions were certainly always in one direction, they varied enormously in extent, according to the direction of the exciting current, the proportion being sometimes as much as 8 to 1. Faradisation of the organs produced no result at all when the coil was so arranged that the make and break shocks were of equal intensity.

It seems to me no longer possible, after these observations, to regard the appearance of "blaze currents" as a specific property of living tissue. It is much more probable that they are to be considered as special manifestations of certain epithelial tissues.

Neither can the presence of exclusively polarisation effects be taken as the sign of the death of a tissue, since these may occur alone in a very pronounced manner in living organs, and in a few organs may even represent the regular and typical effect to stimulation.

In conclusion, it is my pleasant duty to thank Professor Gotch, in whose laboratory these experiments have been carried out, for the facilities afforded me.

"The Specific Heats of Metals and the Relation of Specific Heat to Atomic Weight. Part II." By W. A. TILDEN, D.Sc., F.R.S., Professor of Chemistry in the Royal College of Science, London. Received December 8,—Read December 11, 1902.

(Abstract.)

The following values have been obtained for the mean specific heats of pure aluminium, nickel, cobalt, silver, and platinum, within the several limits of temperature indicated:—

Centigrade.	Aluminium.	Nickel.	Cobalt.	Silver.	Platinum.
−182° to +15°	0·1677	0·0838	0·0822	0·0519	0·0292
− 78 „ +15	0·1984	0·0975	0·0939	0·0550	—
+ 15 „ 100	—	0·1084	0·1030	0·0558	0·0315
15 „ 185	0·2189	0·1101	0·1047	0·0561	—
15 „ 335	0·2247	—	—	—	—
15 „ 350	—	0·1186	0·1087	0·0576	—
15 „ 415	—	0·1227	—	—	—
15 „ 435	0·2356	0·1240	0·1147	0·0581	0·0338
15 „ 550	—	0·1240	0·1209	—	—
15 „ 630	—	0·1246	0·1234	—	—
0 „ 1000	—	—	—	—	0·0377*
0 „ 1177	—	—	—	—	0·0388*

From these results the specific heats at successive temperatures on the absolute scale have been calculated, and it appears that the assumption of a constant atomic heat at absolute zero is untenable.

The mean specific heat of a sample of nickel steel, containing 36 per cent. of nickel and having remarkably small dilatation, was found to be as follows:—

Range of temperature.	Mean specific heat.
−182° to +15°	0·0947
15° „ 100°	0·1204
15° „ 360°	0·1245
15° „ 600°	0·1258

The mean specific heats of the sulphides of nickel and silver were also determined with the object of getting some evidence as to the cause of the difference observed between the two metals in regard to the influence of temperature on their respective specific heats. The following are the values obtained:—

* Violle, 'Comptes Rendus' (1877), vol. 85, p. 543; also 'Phil. Mag.,' [5], vol. 4, p. 318.

Range of temperature.	NiS.	Ag_2S.
−182° to +15°	0·0972	0·0568
15° „ 100°	0·1248	0·0737
15° „ 324°	0·1333	0·0903

The mean value for the specific heat of silver sulphide is less than that for nickel sulphide throughout, but little can be deduced from the results till the influence of temperature on the specific heat of sulphur is known.

"Preliminary Note on the Relationships between Sun-spots and Terrestrial Magnetism." By C. CHREE, Sc.D., LL.D., F.R.S. Received December 18, 1902,—Read January 22, 1903.

(From the National Physical Laboratory.)

I have been engaged during the last two years on an analysis of the magnetic results obtained at Kew Observatory (now the National Physical Laboratory), during an 11-year period, 1890 to 1900. The work has been much interrupted, and is still incomplete. Amongst the points dealt with is the inter-relationship between sun-spot frequency and magnetic phenomena, and, as this has recently been engaging attention elsewhere, I have decided to put certain of my results on record at once. It has long been known from the researches of Balfour Stewart, Ellis, and others, that there is a close connection between the times of occurrence of greatest sun-spot frequency and largest amplitude of the diurnal inequality of magnetic declination and horizontal force. I have investigated whether the numerical relationship between the phenomena can be adequately represented mathematically in a simple way.

A convenient basis for the investigation was presented by the publication by Professor Cleveland Abbe in the 'U.S. Monthly Weather Review,' for November, 1901, of a table of sun-spot frequencies as calculated by Wolf and Wolfer for a very long series of years. After I had carried out all the calculations, Wolfer himself published a similar table* embodying his latest corrections. The differences from Abbe's table are trifling, and mainly confined to two years (1891 and 1892). I judged it best, however, to revise the whole of my arithmetic, so as to employ Wolfer's own most approved figures. In the following remarks S represents Wolfer's value for the sun-spot frequency. The above-mentioned table gives the mean S for each month and for each year.

The magnetic quantity selected for comparison is the mean monthly "range," meaning thereby the difference between the greatest and

* 'Met. Zeitschrift,' May, 1902 p. 195.

least of the twenty-four hourly values in the mean diurnal inequality for the month in question, based on the five *quiet* days selected for the month by the Astronomer Royal. Calling this quantity R for any particular magnetic element, I tentatively assumed

$$R = a + bS \quad \ldots\ldots (1),$$

with a and b constants. I grouped together the 11 Januarys, the 11 Februarys, and so on, of the 11-year period, and determined a and b by least squares for each of the resulting 12 groups. There being only 11 years' data, the calculated values doubtless are appreciably affected by quasi-accidental irregularities, but there is so striking a resemblance between the more conspicuous features of the results found for the declination, inclination and horizontal force as to justify the conclusion that the phenomena are *bonâ fide*. Full particulars will be given later. At present it will suffice to record the mean values found for the a and b of the formula for three groups of months —viz. :—

Winter, comprising November to February,
Equinox „ March, April, September, October, and
Summer „ May to August.

The results are as follows :—

Table I.

	Declination.		Inclination.		Horizontal force. (Unit $1\gamma \equiv 10^{-5}$ C.G.S.)		Vertical force. (Unit $1\gamma \equiv 10^{-5}$ C.G.S.)	
	a.	*b*.	*a*.	*b*.	*a*.	*b*.	*a*.	*b*.
Winter ...	3′·23	0′·0323	0′·63	0′·0105	10·5	0·161	7·0	0·032
Equinox ..	7 ·32	0 ·0478	1·26	0 ·0147	23·5	0·221	17·2	0·026
Summer ..	8 ·91	0 ·0428	1·61	0 ·0137	30·6	0·190	22·7	0·035
Mean.....	6 ·49	0 ·0410	1·17	0 ·0130	21·5	0·191	15·6	0·031

As is obvious from (1), a represents the amplitude of the range corresponding to a total absence of sun-spots. During the eleven years dealt with, Wolfer's mean monthly values for S varied from 0·3 to 129·2, the mean being 41·7.

To bring out more clearly the similarity of the results for the declination, inclination and horizontal force, I have represented the mean value of b for the 12 months in each element by 100. The corresponding values for the three seasons are, then, as follows :—

Table II.

	Winter.	Equinox.	Summer.
Declination	79	117	104
Inclination	81	113	106
Horizontal force.. ...	85	116	99

In obtaining these figures I have retained a figure in the value of b beyond that recorded in Table I.

Tables I and II will suffice to bring out one of the most important points established, viz., that b is certainly different from one month to another, and is, for all the elements except the vertical force, decidedly larger at the equinoxes (more especially it would appear at the spring equinox) than at other seasons. This means that the equinoxes are the seasons at which the amplitude of the diurnal inequality, when considered *absolutely*, is most dependent on the sun-spot frequency. When we take into account, however, the difference between the ranges of the diurnal inequalities at different seasons of the year, we find that winter is the season when sun-spot frequency is *relatively* most important. This will be recognised on reference to Table III, remembering that a represents the range corresponding to a total absence of sun-spots, while $a + 41{\cdot}7\ b$ is the range corresponding to a sun-spot frequency of 41·7, this being, as already mentioned, Wolfer's mean value for the 11 years in question.

Table III.

Values of $41{\cdot}7b \div a$.

	Declination.	Inclination.	Horizontal force.	Vertical force.
Winter	0·42	0·69	0·60	0·19
Equinox	0·27	0·49	0·39	0·06
Summer	0·20	0·35	0·26	0·07

Table III serves also to bring out another important result, viz., that the influence of sun-spot frequency on the amplitude of the diurnal inequality is very much less for the vertical force than for the three other elements considered.

A recent interesting paper by Rajna* shows that the idea of a linear relationship between diurnal magnetic range and sun-spot frequency has already been applied by at least two previous investigators, Rajna and Wolfer. They seem, however, to have applied it only to mean annual values, and to have considered declination only. Rajna, dealing with declination data, observed at Milan over the long period 1836 to 1901, applies a formula of type (1) to what he calls the "medie annuali dell' escursione diurna."

The value he finds for b is 0′·047. He mentions that in an earlier similar investigation, including declination data from several stations, Wolfer obtained the value 0′·040.

I am uncertain as to the precise meaning of Rajna's "medie annuali," but it certainly is not quite the same thing as the mean range in Table I, so that the results are not absolutely comparable.

* 'Rendiconti del R. Ist. Lomb.,' Serie II, vol. 35 1902.

Another recent and able paper bearing on the subject appears in the last published volume of the French Bureau Météorologique, which has just come into my hands. The author, Mr. Alfred Angot, has anticipated me in applying a formula of type (1) to the individual months of the year; but he treats of the amplitude, not of the diurnal range as a whole, but of that of the coefficients of the several terms of the Fourier's series into which the diurnal inequality can be analysed. The paper treats only of the declination—dealing with data from ordinary days at Parc St.-Maur, Greenwich and Batavia—but the author expresses his intention of considering in the future the horizontal force.

A special feature of the present investigation is that the magnetic data are derived exclusively from magnetic *quiet* days. This suggests at once a query and a criticism, a query as to why one did not employ corresponding sun-spot data confined to the magnetically quiet days, a criticism that as the two sets of data employed do not absolutely correspond, the comparison actually made may be misleading.

As to the query: Wolfer, it is true, publishes at regular intervals in the 'Met. Zeitschrift' *provisional* sun-spot frequencies for each day. These figures are, however, presumably inferior in certainty to the final figures he has embodied in his table after consulting all available sources of information. The vital consideration, however, is that at certain seasons of the year there are a number of days for which, owing to the absence of observations, Wolfer has no provisional sun-spot data. With information lacking for two or three out of the five quiet days of a month there would have been a very undesirable amount of uncertainty. As to the criticism, it would be difficult to meet it if it could be held that the enhanced magnetic activity existing at the earth's surface at times of sun-spot maxima is due directly to electrical disturbances in the sun, each disturbance being limited to regions where sun-spots exist, and only those disturbances being effective which happen to be at the moment on the half of the sun visible from the earth. At present I shall only mention the following fact:—I had monthly sun-spot frequencies calculated from Wolfer's *provisional* figures, employing only the five "quiet" days selected for each month by the Astronomer Royal. The mean sun-spot frequency thence deduced for the eleven years (1890 to 1900) differed from the corresponding result given by all Wolfer's days by less than one-fifth of 1 per cent. It is hardly necessary to point out that this fact has an important bearing, not only on the point immediately under consideration, but also on the further question as to the true nature of the connection between sun-spots and magnetic storms.

"Characteristics of Electric Earth-current Disturbances, and their Origin." By J. E. TAYLOR. Communicated by Sir OLIVER LODGE, F.R.S. Received December 16, 1902,—Read January 22, 1903.

The following notes refer to effects which appear to have a distinct connection with the so-called "ionisation" of the upper regions of the atmosphere by radiations from the sun, and which have repeatedly attracted my attention during the course of recent experiments in wireless telegraphy for the British Postal Telegraphs.

In the electronic theory of the causes producing the aurora borealis, it is assumed that by the deflection of the course of the flying ions or electrons towards the poles, due to the earth's magnetic field, a concentration results in those neighbourhoods, giving rise to the phenomenon.

The effects classed by telegraph engineers as earth-currents have also, apparently, a direct connection with the ionisation of the atmosphere. As is well known, these are at times, particularly when auroral displays are in evidence, so strongly pronounced as to interfere more or less with ordinary telegraphic working on earthed circuits.

In special cases, where sensitive apparatus is used, they are, every day, sufficiently pronounced to cause disturbance, for some hours at least, even under normal conditions. They have been found to be particularly troublesome in the Post-office system of wireless telegraphy, in which a sensitive telephone receiver is connected in a low resistance circuit earthed in the sea at both ends.

To enumerate in a systematic manner the various investigations which have been made from time to time on the subject of earth-currents would involve a lengthy paper; but only the more prominent features which have forced themselves on my observation will here be briefly summarised.

The disturbances evidence themselves by producing various characteristic noises in the telephone receiver. They have not been confounded with ordinary telegraphic or other inductive disturbances, as they appear in circuits far removed from any such source of affection. In these latitudes they are always stronger and of more frequent occurrence in summer than in winter. They are daily in evidence for a few hours at or about the time of sunset, *i.e.*, whilst daylight is fading.

In general they do not evidence themselves to any great extent during broad daylight, but are readily precipitated by atmospheric electrical effects or any tendency to thunderstorms, and rarely, if ever, fail to herald the approach of a storm or gale.

The characteristic noises produced may be divided into five classes resembling—

(i) Uniform flowing or rushing of water: this is usually a daytime disturbance, and is occasionally of considerable vigour.

(ii) Intermittent crackling: an accompaniment of other disturbances.

(iii) Bubbling and boiling of water: the usual form of nightfall disturbance, but also frequently occurring in the daytime.

(iv) Rocket disturbances. These are peculiar and characteristic, having some resemblance to the sound produced by a rocket rising in the air. They commence with a shrill whistle and die away in a note of diminishing pitch. They vary in intensity, but always have a similar duration of from 2 to 4 seconds; are freely heard at night, and only occasionally during the day.

(v) Disturbances due to high frequency effects, inaudible on the telephone, but evidenced on the coherer, magnetic detector, or other form of Hertzian receiver.

These various disturbances were, for some time, very puzzling to me; but on perusing Professor J. J. Thomson's paper, read at the Royal Institution on 19th April, 1901, it speedily appeared highly probable that they were due to electrical effects produced in the atmosphere by the ionisation caused by solar radiations and the reaction on this ionisation by electric stresses in the atmosphere. The rocket disturbances, though they are probably not in themselves due solely to ionisation, furnished the first clue to this explanation. They are characteristic of an initial high velocity rapidly damped and ultimately dissipated. They have the same duration as is usually associated with the passage of a meteor across the heavens, and the assumption is that they are actually caused by the passage, in sufficient proximity, of meteoric bodies which set up electrical discharges in the upper rarefied atmosphere, these discharges inducing electric currents in the sea and collected therefrom by the circuit.

Assuming this explanation, it might reasonably be asked why such disturbances are not equally evident during the daytime as at night. The answer lies in the screening effect of the ionised (and therefore conducting) air during the daytime and the absence of such screening at night.

Professor J. J. Thomson has shown, in a modification of the well-known cloud experiment, how the ionisation of a gas may be cleared up or dissipated by an electric field. Doubtless the electric fields to which thunderstorms are due produce similar effects in the atmosphere on nature's gigantic scale. Hence we may expect, as is presumably the case, that the screening referred to above may sometimes be suspended for a time, even during broad daylight, and the rocket disturbances evidenced among others.

Now this assumption of a reaction between the electric stresses in

the atmosphere and the ionisation produced by the sun suggests the source of the other daytime disturbances referred to. It appears highly probable that they are the accompaniment of the clearing up process. Effects analogous to, if not actually, electric currents are doubtless produced in the atmosphere, which are induced in the sea and collected by the circuit. On the other hand, the nightfall disturbances are probably due to normal clearing up processes, revealed when the air becomes sufficiently non-conducting to act no longer as a screen. These suggestions, though by no means complete, are submitted for what they may be worth.

It is probable also that the diurnal variations of the earth's magnetic field are influenced by the same causes.

One more point. The periods of maximum disturbances, experienced on the earthed circuits referred to, appear to coincide with periods of maximum atmospheric disturbances on the newer Hertzian system of wireless telegraphy, and indicate the same source of trouble. Further, I would suggest that we have here a clue to the true explanation of the greater night-time efficiency in signalling observed by Mr. Marconi in recent experiments. With ionised air the electric waves will be partly broken up and absorbed, with consequent abstraction of energy from the transmission. At night, when the ionisation is cleared up, the strength of the radiated waves will be sustained.

Some interesting investigations by the aid of sounds produced, in a telephone, by the passage of electrical currents through rarified gases can no doubt be carried out. Professor Righi has already made some observations in this connection, but much more can yet be done.

"Solar Eclipse of 1900, May 28.—General Discussion of Spectroscopic Results." By J. EVERSHED, F.R.A.S. Communicated by the Joint Permanent Eclipse Committee. Received December 17, 1902,—Read January 22, 1903.

(Abstract.)

In a general way the conclusions arrived at from the discussion of the spectra obtained in 1898 are amply confirmed and extended by the present results. It is now shown that every strong dark line of the solar spectrum exceeding Rowland's intensity 7 is found in these spectra as a bright line; and the great majority of the bright lines of the flash spectrum, excluding hydrogen and helium lines, coincide with dark lines of intensity not less than 3.

Most of the bright arcs of the flash spectrum are well-defined narrow lines admitting of considerable accuracy in the measures, and the present determinations of wave-length indicate that the coincidence of the bright lines with the dark lines is exact within ·05 t.m. for all the well-defined lines.

As regards the relative intensities of the lines of any one element in the flash and Fraunhofer spectra, my previous results require modification and extension as follows: The relative intensities of isolated lines of an element in the flash spectrum are in general, but not exact, agreement with those of the same element in the solar spectrum, and those lines which are exceptionally strong in the flash are in most cases lines which are enhanced in the spark spectrum of the element.

All of the more prominent enhanced lines of iron and titanium, as determined by Sir Norman Lockyer, are found to coincide with strong lines in the flash, but owing to the compound nature of some of the lines, it is not certain that all of these have abnormal intensities in the flash.

There is no evidence of differences in the relative intensities of the lines of an element in the higher or lower regions of the flash layer, and the enhanced lines appear to predominate throughout the entire depth of the radiating stratum. The enhanced lines are equally prominent in the polar regions and in low latitudes, and the flash spectrum generally is now found to be the same in all latitudes and shows no essential change after an interval of five years.

An explanation of the abnormal intensities of the enhanced lines in the flash spectrum is now offered, which depends on the assumption of a continuous circulation of the solar gases in a radial direction; the highly heated ascending gases giving the predominant features to the flash spectrum, whilst the cooler more diffused gases, slowly subsiding, determine the character of the absorption spectrum.

The entire chromosphere is supposed to consist of innumerable small eruptions or jets of highly-heated gases similar to the so-called "metallic" prominences, which are only the more pronounced manifestations of the same eruptive agencies.

Evidence for this is found in the characteristic features of the chromosphere, and in the detailed structure of many of the Fraunhofer lines, which show wide emission lines underlying the narrow absorption lines. These ill-defined bright lines in the normal solar spectrum are distinctly displaced towards the violet, indicating a strong uprush of the hotter gases, whilst the narrow absorption lines are almost in their normal positions, and appear to indicate a slow and uniform descent of the absorbing gases.

The final conclusion is that the flash spectrum represents the emission of both ascending and descending gases, whilst the Fraunhofer spectrum represents the absorption of the descending gases only.

"On the Electrodynamic and Thermal Relations of Energy of Magnetisation." By J. LARMOR, M.A., D.Sc., Sec. R.S. Received January 2,—Read January 22, 1903.

1. There appears to be still some uncertainty as to the principles on which the energy of magnetised iron is to be estimated, and the extent to which that energy is electrodynamically effective. The following considerations are submitted as a contribution towards definite theoretical views.

The electrokinetic energy of a system of electric currents $\iota_1, \iota_2, \ldots$, flowing in complete linear circuits in free aether, is known to be

$$\tfrac{1}{2}(\iota_1 \mathrm{N}_1 + \iota_2 \mathrm{N}_2 + \ldots);$$

wherein N_1 is the number of tubes of the magnetic force (α, β, γ) that thread the circuit ι_1, and is thus equal to $\int (l\alpha + m\beta + n\gamma) dS$ extended over any barrier surface S which blocks that circuit, (α, β, γ) being circuital (*i.e.*, a stream vector) so that all such barriers give the same result. As under steady circumstances (α, β, γ) is also derivable from a magnetic potential V, which has a cyclic constant $4\pi\iota$ with regard to each current, this energy assumes the form

$$\frac{1}{8\pi} \Sigma \int \mathrm{V} \left(l \frac{\partial \mathrm{V}}{\partial x} + m \frac{\partial \mathrm{V}}{\partial y} + n \frac{\partial \mathrm{V}}{\partial z} \right) d\mathrm{S},$$

in which the integrals are now extended over both faces of each

barrier surface. This is equal by Green's theorem to the volume-integral

$$\frac{1}{8\pi}\int(\alpha^2+\beta^2+\gamma^2)\,d\tau$$

extended throughout all space. This latter integral is in fact taken in most forms of Maxwell's theory to represent the actual distribution, in all circumstances whether steady or not,* of the electrokinetic energy among the elements of volume of the aether, in which it is supposed to reside as kinetic energy.

2. The most definite and consistent way to treat magnetism and its energy is to consider it as consisting in molecular electric currents; so that in magnetic media we have the ordinary finite currents, combined with molecular currents so numerous and irregularly orientated that we can only average them up into so much polarisation per unit volume of the space they occupy. So far in fact as the latter currents are concerned, the only energy that need or can occupy our attention is that connected with some regularity in their orientation, *i.e.*, with magnetisation, the remaining irregular part being classed with heat. If there were no such molecular currents, the magnetic force (α, β, γ) in the aether would in steady fields be derived from a potential cyclic only with regard to the definite number of circuits of the ordinary currents. But when magnetism is present this potential is cyclic also with respect to the indefinitely great number of molecular circuits. The line integral of magnetic force round any circuit is $4\pi(\Sigma\iota+\Sigma\iota')$, where $\Sigma\iota'$ refers to the practically continuous distribution of magnetic molecular currents that the circuit threads. This latter vanishes when these currents are not orientated with some kind of regularity. If we extend the integral from a single line to an average across a filament or tube of uniform cross-section δS, with that line for axis, by multiplication by δS, we obtain readily the formula

$$\delta S\int(\alpha dx+\beta dy+\gamma dz) = \delta S 4\pi\Sigma\iota + 4\pi\int(A dx+B dy+C dz)\delta S$$

in which $(A, B, C)\delta\tau$ represents the magnetisation in volume $\delta\tau$. Thus, after transposition of the last term, and removal of the factor δS after the average has now been taken, we obtain

$$\int\{(\alpha-4\pi A)\,dx+(\beta-4\pi B)\,dy+(\gamma-4\pi C)\,dz\} = 4\pi\Sigma\iota$$

In other words this new vector $(\alpha-4\pi A, \beta-4\pi B, \gamma-4\pi C)$, is *derived from a potential* which is cyclic in the usual manner with regard to the ordinary currents *alone*.

* In the previous electric specification, the fictitious electric currents of aethereal displacement must be introduced when the state is not steady.

If we compare this result with the customary magnetic vectors of Kelvin and Maxwell, it appears that (α, β, γ) must represent the "induction," and so will hereafter be denoted, after Maxwell, by (a, b, c). The new vector, which has a potential cyclic with respect to the finite currents only, represents the "force," and will hereafter be denoted by (α, β, γ), whose significance is thus changed from henceforth. The "induction" on the other hand has not necessarily a potential, but is, by the constitution of the free aether, always circuital; that is, it satisfies the condition of streaming flow

$$\frac{\partial a}{\partial x}+\frac{\partial b}{\partial y}+\frac{\partial c}{\partial z}=0.$$

The expression for the energy now includes terms

$$\tfrac{1}{2}(\iota_1 \mathrm{N}_1+\iota_2 \mathrm{N}_2+\ldots)$$

for the ordinary currents $\iota_1, \iota_2, \ldots$, where $\mathrm{N}_1, \mathrm{N}_2, \ldots$ are the fluxes, *of magnetic induction*, through their circuits; this transforms as usual into

$$\frac{1}{8\pi}\Sigma\int \mathrm{V}(la+mb+nc)\,d\mathrm{S}$$

over both faces of each barrier, which by Green's theorem is equal to

$$\frac{1}{8\pi}\int(a\alpha+b\beta+c\gamma)\,d\tau \quad \ldots\ldots\ldots\ldots\ldots \text{(i)}$$

extended throughout all space. But there are also terms

$$\tfrac{1}{2}(\iota_1'\mathrm{N}_1'+\iota_2'\mathrm{N}_2'+\ldots)$$

for the molecular currents; now taking N′ to be the cross-section of the circuit multiplied by the component of the averaged induction normal to its plane, and remembering that ι' multiplied by this cross-section is the magnetic moment of this molecular current, it appears that $\iota'\mathrm{N}'$ is equal to the magnetic induction multiplied by the component of the magnetic moment in its direction, and therefore $\frac{1}{2}\Sigma\iota'\mathrm{N}'$ is equal to

$$\tfrac{1}{2}\int(\mathrm{A}a+\mathrm{B}b+\mathrm{C}c)\,d\tau.$$

Thus the magnetic circuits add to the energy the amount*

$$\tfrac{1}{2}\int(\mathrm{A}\alpha+\mathrm{B}\beta+\mathrm{C}\gamma)\,d\tau \quad \ldots\ldots\ldots\ldots\ldots \text{(ii)}$$

together with

$$2\pi\int(\mathrm{A}^2+\mathrm{B}^2+\mathrm{C}^2)\,d\tau \ldots\ldots\ldots\ldots\ldots \text{(iii)}$$

* [These energies as here determined are *kinetic*; if they are (as is customary) to be considered as *potential*, their signs must be changed. *Cf.* 'Phil. Trans.,' A, 1894, p. 806.]

The formula (i) is usually taken, after Maxwell's example, to represent the energy of the electrokinetic field. It here appears that it represents only the part of the energy that is concerned with the currents, arising from their mutual interactions and the interactions of the magnets with them: that there exists *in addition* a quantity (ii) which is that taken by Maxwell as the energy of magnetisation in the field (α, β, γ), and also a quantity (iii), which is purely local and constitutive, of the same general type as energy of crystallisation. The question arises whether (iii) is a part of the intrinsic energy of magnetisation of different kind from (ii), in that it cannot even partially emerge as mechanical work, or on the contrary the usual formula (ii) must be amended. See §§ 5, 8. In any case the dynamics of the field of currents (when there are no irreversible features) involves only that part of the energy function in which the currents operate, thus excluding both (ii) and (iii).

3. The simplest example is that of a coil of n turns carrying a current ι, wound uniformly on a narrow iron ring-core, of cross-section S and length l. On the present basis the energy is made up of an electrodynamic part $\frac{1}{2}\iota n\mathfrak{B}\mathrm{S}$ and a magnetic part $\frac{1}{2}\mathfrak{H}\mathfrak{B}\mathrm{S}l$; as $4\pi n\iota = \mathfrak{H}l$ by the Amperean circuital law, these parts are

$$\frac{1}{8\pi}\mathfrak{B}\mathfrak{H}v \quad \text{and} \quad \tfrac{1}{2}\mathfrak{B}\mathfrak{H}v,$$

when v is the volume of the core; they make up *in all* $\mathfrak{B}^2/8\pi$ per unit volume instead of the usual $\mathfrak{B}\mathfrak{H}/8\pi$. The former part is mechanically available. The question has been raised by Lord Rayleigh* whether the latter part, which includes the very large term (iii) above, namely $2\pi\mathfrak{J}^2v$, in the case of iron, has any considerable mechanical effectiveness; the question can only arise when it belongs in part to *permanent magnetism* whose ultimate annulment can induce a current,—when the current vanishes the energy of permanent magnetism, in the present case represented by (iii) alone, is the only part of the energy of the system that remains. The conclusion reached by him is that it cannot be annulled quick enough, when the ring carries a coil, to develope any considerable available electric energy by induction.

4. We may form a rough illustration of the mechanical *rôle* of this purely magnetic energy by considering, as the analogy of the currents, a branching system or network of pipes carrying liquids, in one of which a turbine is located, to be driven by the stream, which will be supposed to be an alternating one. The flow will be directed more fully into this particular pipe, and higher pressure will also be attained, after the manner of the hydraulic ram, if it communicates at the side with an expansible reservoir into which the liquid can readily

* 'Phil. Mag.,' 1885: also 'Archives néerlandaises, vol. 2, 1891, p. 6, reprinted in 'Phil. Mag.,' 1902, and in 'Scientific Papers,' vol. 4, No. 272.

force its way, to be expelled again by the elasticity of its walls when the stream begins to set in the reverse direction. This increase of kinetic pressure on the turbine roughly represents the electromotive pressure on a motor due to the increased magnetic flux, and the energy spent in expanding the reservoir as it fills up represents the energy of magnetisation of the iron. If things were perfectly reversible in the reservoir, that is if the iron were perfectly soft, the latter energy would rise and fall concomitantly with the alternations of pressure on the motor, but of course if its temperature remained constant it would contribute nothing to the energy driving the motor, which must be introduced into the system from an extraneous source. But if there are frictional resistances involved in filling the reservoir, the operations will not be perfectly reversible, and mechanical energy will be lost in it by conversion into heat; and moreover on account of the phase of its changes getting out of step—still more by permanent delays such as are classed under hysteresis—it will operate less efficiently in directing the stream of energy towards the turbine. Both these statements have analogical application to the iron in a magnetic circuit.

An example is provided by the ring-coil aforesaid. Suppose that when the current has ceased in the coil the core retains permanent magnetism, its energy being the latter term in the formula above. This corresponds to the reservoir becoming temporarily choked, so that it retains its contents after the pressure that drove the liquid into it has been removed. The question arises whether this retained energy is available for mechanical work. The present aspect of the matter appears to lead to the conclusion (Lord Rayleigh's) that it will not be available to any considerable extent unless its pressure in the reservoir is considerable, that is, in the magnetic case, unless the iron is not very receptive of magnetisation.

The paradox that energy of residual magnetism, which is outside the electrokinetic system, can on running down affect that system, shows that the circumstances are more general than an analogy of a pure dynamical system of finite number of degrees of freedom can illustrate. In fact the equations of dynamics imply permanent structure of the system; whereas in Professor Ewing's illustrative model of paramagnetisation, when the displacement is great enough the structure changes by the component magnets toppling over,* and after the general disturbance thus set up has subsided with irrecoverable loss of energy into heat, there remains a new structure to deal with. The only way to estimate the available part that may be latent in the great store of energy of residual magnetism of an iron core is thus by the empirical process of detailed experiment. Lord Rayleigh has inferred

* The effective susceptibility dI/dH becoming enormous in the steep part of the characteristic curve.

from the form of the curve of hysteresis for retentive iron in high fields that the fraction that is directly available at the actual temperature must always be small, and he supports the inference by considerations of the nature of the above analogy; in the absence of hysteresis there would be no such direct availability. He derives the practical result that a complete magnetic circuit is deleterious for induction coils in which length of spark is the *desideratum*, the increased total induction attained inside the ring-core being more then neutralised by the diminished promptness of magnetic reversal.

In fact, if the core, laminated so as to have merely negligible conductivity, is surrounded by a perfectly conducting coil or sheath, and its permanent magnetism is removed at constant rate $-dI/dt$ by an ideal process applied to it, the intensity of induction in the core will diminish at the rate $-4\pi dI/dt$; and this defect of induction must be made good by the influence of the current thereby induced in the sheath, as otherwise there would be a finite electromotive power in it, which is impossible on account of its perfect conductivity. This restored induction is of the form $H' + 4\pi I'$, where I' is the magnetism induced by the force H' due to the induced current; thus

$$\frac{d}{dt}(H' + 4\pi I') + 4\pi \frac{dI}{dt} = 0,$$

and the actual total rate of fall of magnetisation is diminished to $\frac{dI}{dt} - \frac{dI'}{dt}$, which is only the fraction $\frac{1}{4}\pi \frac{dH'}{dt} \Big/ \frac{dI'}{dt}$ of the constrained loss of retained magnetism dI/dt. In this most favourable case the action of the coil or sheath thus delays the time-rate of loss of permanent magnetism in the core in the ratio $(4\pi\kappa')^{-1}$, where κ' is the effective permeability for small additional force under the actual circumstances; that is, the delay in reversal more than compensates the gain in induction.

5. There remains another question, when viscous and other hysteretic effects are practically absent so that the changes of magnetisation exactly keep step with those of the currents, and the degree of availability of *residual* magnetic energy thus does not arise;—whether the energy of the magnetisation comes from the store of heat of the material and is thus concomitant with a cooling effect when no heat is supplied, or whether it is in part intrinsic inalienable energy of the individual molecules merely temporarily classed as magnetic. So far as it may be the latter, it must for each element of volume depend on the state of that element alone, like the part (iii) of § 2. It has already been seen that no part of (ii) or (iii) can be supplied from the electrodynamic field. This points to the intrinsic energy of paramagnetism, except an unknown fraction of the local part (iii), which depends only on the state of polarisation of the element of the medium, being

derived from purely thermal sources; and the following thermodynamic argument* will strengthen this conclusion.

If the value of the magnetic susceptibility κ for any material is a function of the temperature, we can perform a Carnot reversible cycle by moving a small portion (say a sphere) of the substance in the permanent field of a system of magnets supposed held rigidly magnetised by constraints. We can move it into a stronger region H_2 of this field, of varying strength H, maintaining it at the temperature θ by a supply of heat from outside bodies at that temperature; we can then move it on further, having stopped the supply of heat, until its temperature becomes $\theta - \delta\theta$; we can move it back again isothermally by aid of a sink of heat at this temperature until the stage H_1 is reached, when further progress back adiabatically will restore it to its original condition. If κ is a function only of the strength of field and of the temperature, this cycle will be reversible. If E is the heat-energy supplied at temperature θ, and W is the work done on the sphere by external bodies in the cycle, the principle of Carnot gives the relation

$$\frac{E}{\theta} = \frac{W}{\delta\theta}.$$

Now
$$W = -\frac{d}{d\theta}(\tfrac{1}{2}I_2H_2 - \tfrac{1}{2}I_1H_1)\,\delta\theta$$

$$= -\tfrac{1}{2}\frac{d\kappa}{d\theta}(H_2^2 - H_1^2)\,\delta\theta, \text{ if } \kappa \text{ is small,}\dagger$$

when the cycle is taken such that the change of H along the adiabatic

* This theoretical deduction of Curie's law has been already given substantially in 'Phil. Trans.,' A, vol. 190, 1897, p. 287.

The theory of diamagnetism, which assigns it to modification of conformation in the individual molecule by the inducing field rather than to average spacial orientation of the crowd of molecules, leads to a non-thermal origin as regards that part. The analogous question (*loc. cit.*) as to whether dielectric polarisation is mainly an affair of orientation of unaltered molecules like paramagnetism, or one of polarity due to internal deformation of the molecule like diamagnetism, is now answered by the experiments of J. Curie and Compan ('Comptes Rendus,' June 2, 1902). It appears that the dielectric coefficient of glass, for rapid changes, *diminishes*, but not very quickly, with fall of temperature, and that at temperatures below $-70°$ C. duration of charge ceases to have influence on its value. The electric excitation is thus analogous to diamagnetism and has no thermal bearing, its energy being self-contained in the molecule; the signs of the susceptibilities in the two cases are different, because the one is of static, the other of kinetic character. The sharpness of the Zeeman magneto-optic effect has already led ('Aether and Matter,' 1900, p. 351) in this direction, for it requires that the electric polarisation in the molecule shall be of isotropic type, so that there may be no axis of maximum susceptibility.

† This restriction is not necessary for the final result; if κ is not small, W and E have both to be multiplied by the same factor.

part of the path is negligible compound with that along the isothermal part. Thus

$$E = -\tfrac{1}{2}(H_2^2 - H_1^2)\,\theta\,\frac{d\kappa}{d\theta}.$$

Now the experiments of Curie on the relation of κ to θ in weakly *paramagnetic* materials make κ vary inversely as θ; and this result has more recently been verified down to very low temperatures by Dewar and Fleming. This gives

$$E = \tfrac{1}{2}\kappa(H_2^2 - H_1^2).$$

Thus the movement of the magnetisable material at uniform temperature is accompanied by a supply to it of heat, equal to the mechanical work done by it owing to the attraction of the field; and this heat is just what is wanted to be transformed into the additional energy of intrinsic magnetisation (ii) of § 2. It is to be observed that in the actual experiments κ was small, and the other part (iii) of this energy therefore negligible: so that no conclusion as to the extent to which its source is thermal can be derived from Curie's law.

6. The uncertainties of § 4 do not of course affect the estimation of the loss of motive power arising from cyclic magnetic hysteresis, for we have here to do with the *mutual* energy of the applied field and the magnet, not the intrinsic local energy of the latter by itself. If the applied field is (α, β, γ), the total energy employed in polarising the magnetic molecules in volume $\delta\tau$ is

$$(A\alpha + B\beta + C\gamma)\,\delta\tau.$$

So long as the polarisation is slowly effected against the resilience of reversible internal elastic forces this is stored as potential energy; but any want of reversibility involves degradation of some of it into heat, while if the field were instantaneously annihilated the molecules would swing back and vibrate, so that ultimately all would go into heat.

Let us pass the magnetic body through a cycle by moving it around a path in a permanent magnetic field (α, β, γ). An infinitesimal displacement of the volume $\delta\tau$ from a place where the field is (α, β, γ) to one where it is $(\alpha + \delta\alpha, \beta + \delta\beta, \gamma + \delta\gamma)$ does mechanical work, arising from the magnetic attraction, of amount

$$(A\delta\alpha + B\delta\beta + C\delta\gamma)\,\delta\tau.$$

The integral of this throughout the whole connected system gives the virtual work for that displacement, from which the forces assisting it are derived as usual. Confining attention to the element $\delta\tau$ the work supplied by it from the field, to outside systems which it drives, in traversing any path is thus

$$\delta\tau \int (A\,d\alpha + B\,d\beta + C\,d\gamma),$$

the integral being taken along the path. If (A, B, C) is a function of (α, β, γ), that is if the magnetism is in part thoroughly permanent, and in part induced without hysteresis, so that the operation is reversible, this work must vanish for a complete cycle; otherwise energy would inevitably be created either in the direct path or else in the reversed one of the complete system of which $\delta\tau$ is a part. Thus the negation of perpetual motion in that case demands that

$$\mathrm{A}d\alpha + \mathrm{B}d\beta + \mathrm{C}d\gamma = d\phi,$$

where ϕ is a function of (α, β, γ), involving only even powers, and practically quadratic for small fields. Its coefficients are then the six magnetic constants for general aeolotropic material, no rotational quality in the magnetisation being thus allowable by the doctrine of energy. But if there is hysteresis, so that the cycle is not reversible,

$$-\delta\tau \int (\mathrm{A}d\alpha + \mathrm{B}d\beta + \mathrm{C}d\gamma),$$

or in vector product form $-\delta\tau \int \mathfrak{H}\, d\mathfrak{B}$, represents negative mechanical work done, or energy degraded, in the cycle.

In addition to this energy concerned with attraction, the external field expends energy in polarising or orientating the individual molecules against the internal forces of the medium, of aggregate amount

$$\delta\tau \int (\alpha d\mathrm{A} + \beta d\mathrm{B} + \gamma d\mathrm{C}).$$

In any case, whatever the hysteresis, the sum of this second part and the first reversed is integrable independently of the path, giving

$$\delta\tau \,|\, \mathrm{A}\alpha + \mathrm{B}\beta + \mathrm{C}\gamma \,|,$$

namely, the change in the total energy in the element, thus vanishing for a cycle which restores things to their original state, as it ought to do The latter part is purely internal, and of merely thermal value as in § 5. The former part represents the averaged waste of direct mechanical energy in moving the iron armature through the cycle, and accounts for the heat thus evolved. It is the expression of Warburg and of Ewing for magneto-hysteretic waste of mechanical energy in driving electric engines; for a portion of a cycle it represents work partly degraded and partly stored magnetically.

7. Reverting to § 5, we may profitably illustrate by working out into detail a suggestion of Lord Rayleigh (*loc. cit.*). Consider a ring-coil of n turns with a flexible open core of soft iron of length l and cross-section S, whose flat ends are bent round until they face each other at a distance small compared with the diameter of section. We can apply Hopkinson's theory of the open magnetic circuit to trace the

transformation of the energy of attraction between these poles of the core, into electrokinetic energy of the coil, as the poles close up together. As frictional waste is not essential to this question, we can consider the coil to be a perfect conductor which will store all the energy without loss. We need not postulate that the iron is of constant permeability or devoid of hysteresis. When the distance between the pole-faces is x, let the current in the coil be ι. The total energy is

$$\tfrac{1}{2}n\iota\mathrm{N} + \text{energy of magnetisation};$$

and part of the latter may remain sub-permanent when ι vanishes. The principle of the magnetic circuit gives, as $\mathfrak{B}\mathrm{S} = \mathrm{N}$, the formula

$$\frac{\mathrm{N}}{\mu\mathrm{S}}l + \frac{\mathrm{N}}{\mathrm{S}}x = 4\pi n\iota,$$

assuming as usual that the lines of magnetic force are conveyed straight across the air-gap between the pole-faces. Thus the electromagnetic energy T, equal to $\frac{1}{2}n\iota\mathrm{N}$, is

$$2\pi\frac{n^2\iota^2\mathrm{S}}{x + l/\mu};$$

and when x is diminished by $-\delta x$, its increment is approximately

$$-2\pi\frac{n^2\iota^2\mathrm{S}}{(l/\mu)^2}\delta x + 4\pi\frac{n^2\iota\mathrm{S}}{l/\mu}\delta\iota \quad \text{.....(i)}$$

In this displacement the work expended from the electrodynamic system in mechanical attraction between the poles magnetised to intensity I, equal to $\kappa\mathrm{N}/\mu$, is $-\mathrm{IS}2\pi\mathrm{I}\delta x$, which is

$$-\left(1 - \frac{1}{\mu}\right)^2 2\pi\frac{n^2\iota^2\mathrm{S}}{(l/\mu)^2}\delta x \quad \text{.....(ii)}$$

Comparing (ii) with (i) it appears that in cases for which μ is great, to which alone the principle of the magnetic circuit can be applied, the work of mechanical attraction by which the pole-faces can *transfer* potential energy to a spring placed between them, by compressing it, is concomitant with equal increase of the electrokinetic energy if the current do not change. As there is no source of energy, the current must therefore vary, and so that the total change of electrokinetic energy given by (i) and (ii) vanishes; that is, it must diminish by $-\delta\iota$, given by $\delta\iota = \frac{1}{2}\frac{\mu\iota}{l}(1 + (1 - \frac{1}{\mu})^2\}\delta x$, which is practically equivalent to $\delta\iota = \dfrac{\mu\iota}{l}\delta x$.

This result may be immediately verified by the Lagrangian process. As there is infinitely small resistance, the electric pressure $\dfrac{d}{dt}\dfrac{\partial\mathrm{T}}{\partial\dot{\iota}}$ in

the coil must be infinitely small; hence $\iota \propto x + l/\mu$, so that $\frac{\delta\iota}{\delta x} = \frac{\mu}{l}$ when x is negligible.

The energy of magnetisation of the core, which is $\frac{1}{2}\mathfrak{J}\mathfrak{B}l\mathrm{S}$, where $\mathfrak{J} = \kappa\mathfrak{B}/\mu$, and therefore is $\frac{1}{2}\frac{\kappa}{\mu}\left(\frac{4\pi n\iota}{x + l/\mu}\right)^2 l\mathrm{S}$, is not included in this conservation. Its increase for a change δx is $-\frac{\mu - 1}{4\pi}\left(\frac{4\pi n\iota}{l/\mu}\right)^2 \mathrm{S}\delta x$, which is large compared with the quantities above. The fraction μ^{-1} of it, which is comparable with the other variations, is compensated *thermally*, by absorption of the heat of the system, and has, therefore, only the limited availability of thermal energy. The remainder belongs intrinsically to the magnetisation, constituting mutual energy of contiguous molecules; how much of it, as above expressed, is of thermal origin remains undetermined in the absence of calorimetric experiment.

8. The main points that it has been sought to bring out are as follows:—

(i) In an electrodynamic field there exists the usual specification of electrokinetic energy, but also *in addition* the energy of magnetisation of magnetic material.

(ii) This energy of magnetisation appears as made up of a part given by the ordinary formula, which (when paramagnetic) is derived from thermal sources, and so in the absence of hysteresis has the limited mechanical availability of thermal energy; together with a local part which is to some extent thus available, but is also in part permanent intrinsic energy of the molecules, regarded temporarily as magnetic energy.

(iii) The law of Curie, that the susceptibility of weak paramagnetic substances is inversely proportional to the absolute temperature, is involved in these statements.

(iv) The extent of the direct (non-thermal) availability of *retained* magnetism can be inferred only by empirical procedure, for example, in general features by inspection of the hysteresis diagram, as pointed out by Lord Rayleigh.

"The Spectrum of γ Cygni." By Sir NORMAN LOCKYER, K.C.B., F.R.S., and F. E. BAXANDALL, A.R.C.Sc. Received December 11,—Read December 11, 1902.

(Abstract.)

The paper gives an account of the investigation of the Spectrum of γ Cygni in relation to other celestial and terrestrial spectra. It is pointed out that this spectrum—which is of the Polarian type in the Kensington classification—is the connecting link between the spectrum of the Aldebarian stars, in which the arc lines of the metallic elements predominate, and that of α Cygni, chiefly composed of the enhanced lines of some of the metals. These two sets of lines are of about equal prominence in the γ Cygni spectrum.

It is also shown that, in regard to the relative intensities of the metallic and proto-metallic lines, there is a close resemblance between the spectrum of γ Cygni and that of the chromosphere, thus indicating that the temperature and electrical conditions prevailing in the two light sources are nearly identical. A comparison, however, of the intensities of the stronger arc and enhanced lines in the two spectra tends to show that the chromosphere is, if anything, of a slightly higher temperature.

It is claimed that the majority of the lines of γ Cygni—and therefore, also, of the chromosphere—are due to metallic vapours, and that there is little evidence to support Professor Dewar's suggestion that most of the 339 chromospheric lines, recorded by Humphreys in his eclipse results, can be accounted for as probably being due to the rarer atmospheric gases.

The relation of the γ Cygni spectrum to that of α Cygni is also discussed in detail.

At the end of the paper a table is given showing the wave-lengths, intensities, and probable origins of the lines which occur in the photographic spectrum of γ Cygni, taken with a 6″ Henry prism of 45° angle. The lines are compared with those of the chromosphere, δ Canis Majoris (Pickering's record), and α Cygni.

"Some Dielectric Properties of Solid Glycerine." By ERNEST WILSON, Professor of Electrical Engineering, King's College, London. Communicated by Sir WILLIAM PREECE, K.C.B., F.R.S. Received January 7,—Read January 22, 1903.

At high frequency, obtained by resonance, Thwing gives 56·2 for the specific inductive capacity of glycerine at 15° C.* The specific inductive capacity of pure liquid glycerine at 8° C. was found to be about 60, whether the frequency of experiment was of the order 2 million or 100.† At a frequency of 120 the specific capacity of glycerine varies from about 60 to 4·6, as the temperature is varied from −50° to −100° in platinum degrees.‡ The present paper deals with the specific capacity of glycerine at temperatures varying from about +10° to −80° C. The methods of experiment and the apparatus are those described in connection with the earlier experiments.† The platinum plates forming the electrodes of a condenser were placed in pure glycerine, supplied by Messrs. Hopkins and Williams, and after a preliminary experiment, made to find its conductivity (see table), the glycerine was frozen by aid of carbonic acid snow. The temperature of the glycerine was observed by aid of a platinum thermometer. At −48° C. the specific capacity at about 2 million periods per second is 3·97. At −44° C. the specific capacity at about 100 periods per second is 54, which is of the same order of magnitude as that given by Fleming and Dewar. The conductivity of this condenser at −59° C., and with times of contact varying from 0·00002 to 0·009 second, was found and is shown by the curve No. 1 in the accompanying figure. If K be the instantaneous capacity and C the electric resistance, then the total capacity of the condenser at time t is

$$K + \int_0^t \frac{1}{C}\,dt - \frac{1}{C_\infty}t.$$

Now the capacity of this condenser at −48° C. at about 2 million periods per second is 0·00032 micro-farad, and the value of $\int_{0{\cdot}00002}^{0{\cdot}009} \frac{1}{C}\,dt - \frac{1}{C_\infty}\,t$, as given by the curve, will only account for 0·00018 micro-farad. Assuming that it will be the residual charge which comes out in one-sixth of the period which produces the effect on the capacity, a large proportion of the residual charge comes out between the times $\frac{1}{12} \times 10^{-6}$ and 20×10^{-6} second. If the refractive index of the glycerine when frozen is taken to be 1·47 for short times, then Maxwell's law would indicate a specific capacity of 2·17; and

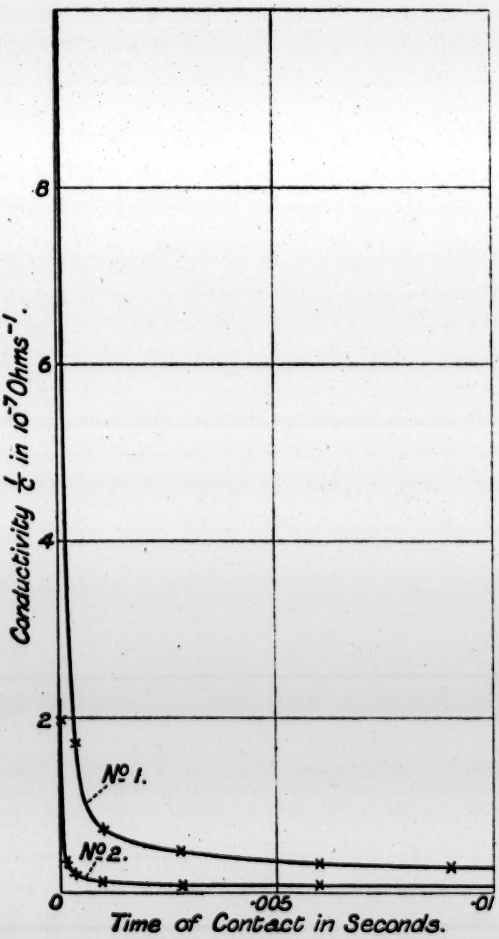
* 'Zeitschrift für Physikalische Chemie,' vol. 14, p. 293.

† Hopkinson and Wilson, 'Phil. Trans.,' A, vol. 189 (1897), pp. 109—136.

‡ Fleming and Dewar, 'Proc. Roy. Soc.,' vol. 61, p. 316.

3·97 was observed at 2 million periods per second. It is possible, therefore, that some residual charge has already come out at 2 million periods per second.

When the glycerine was warmed up to atmospheric temperature 10° C., it still remained *solid*, and its dielectric properties were examined at this temperature. The resistance of the condenser was 216000 ohms, whether the time of contact was 0·00002 or 0·011 second, and this is about $3\frac{1}{4}$ times the resistance at 10° C. when the glycerine was in the *liquid* state. No residual charge comes out between the above times. At 2 million periods per second the specific capacity of this solid glycerine at 10° C. is 6·67. By the method described previously* the specific capacity is 16 at 50 periods per second. The specific capacity in the liquid and solid states at 10° C. varies approximately as the conductivity for long times. The residual charge comes out at times less than 0·00002 second, whether the glycerine be liquid or solid at 10° C.



Finally, the condenser was frozen in carbonic acid snow in ether, and the conductivity at −81° C. is shown by the curve No. 2 in the figure. The final conductivity is less, and the area is less than given by the curve No. 1. This difference is due to the lower temperature for curve No. 2. Fleming and Dewar have shown that the specific capacity of glycerine for long times changes very rapidly with its temperature between −50° and −100° Pt. The specific capacity at −81° C. was found to be 3·8 at 2 million periods per second.

During the freezing processes above referred to the conductivity of the glycerine was observed at different temperatures. The results are given in the accompanying table. It is noteworthy that for time 0·00002 second

* Hopkinson and Wilson, 'Phil. Trans.,' A, vol. 189 (1897), p. 118.

the conductivity falls and then rises, as the temperature is varied from +13° to −59° C. when freezing from the liquid to the solid state. An analogous effect has been observed in soda-lime glass.*

When the time of contact is 0·006 second, all the points in the table lie well on the same curve between −26° and −81° C. It is when freezing from the liquid to the solid states that such serious changes occur at the short times.

Time of contact in seconds after application of force.	Conductivity in 10^{-6} ohms^{-1}.						
	Freezing from liquid to solid state.			Freezing from solid state.			
	+13° C.	−26° C.	−59° C.	+12° C.	−48° C.	−75° C.	−81° C.
0·00002	15	1·72	8·03	4·63	0·388	0·191	0·197
0·00017	..	..	..	..	..	..	0·0301
0·00035	..	..	0·172	..	..	..	0·0201
0·00099	..	..	0·0708	..	..	..	0·0120
0·0028	..	..	0·0481	..	..	..	0·0109
0·0060	..	..	0·0319	..	0·134	0·012	0·00926
0·0090	15	0·67	0·0287	..			
0·011	..	..	0·0249	4·63			
0·018	..	..	0·0160				
			Curve No. 1				Curve No. 2

* Hopkinson and Wilson, 'Phil. Trans.,' A, vol. 189 (1897), pp. 109—136.

"The Relation between Solar Prominences and Terrestrial Magnetism." By Sir NORMAN LOCKYER, K.C.B., F.R.S., and WILLIAM J. S. LOCKYER, M.A., Ph.D., F.R.A.S. Received January 14,—Read January 29, 1903.

[PLATES 4 AND 5.]

It has been stated in a previous communication* that a preliminary reduction of the Roman observations of prominences, observed on the sun's limb by Tacchini, indicated that, in addition to main epochs of maxima and minima of prominences coinciding in time with those of the maxima and minima of the total spotted area, there are also prominent subsidiary maxima and minima.

One of us has pointed out in a recent communication to the Académie des Sciences† that a comparison of the frequency of prominences visible in each solar latitude with the frequency of the most intense magnetic storms, indicated that (*a*) magnetic storms classed as "great" by Ellis, and the greatest prominence activity near the poles of the sun occurred at the same time; and (*b*) that the curve of general magnetic activity was nearly the same as that of the prominences observed near the solar equator.

The object of the present communication is to give a more detailed account of the research so far as it has gone.

The Observations of Prominences.

The fine series of observations, made by Tacchini, of the numbers and latitudes of prominences seen on the sun's limb was used as a basis for the curves discussed. These observations were commenced in 1872, and have been continued up to the present day, so that we have a valuable continuous record. They have been published‡ from time to time in full detail, thus rendering it possible to deal with them in any manner that may be desired. In the reduction of the observations each zone of 10° was examined and discussed by itself. The observations were divided in the first instance into groups of three months, and the percentage frequency of the prominences was determined by dividing the number observed by the number of days on which observations were made in this period.

In this way a set of eighteen curves, nine for each hemisphere, was made, showing the variation from year to year of the percentage frequency of prominence activity in each ten-degree zone.

* 'Roy. Soc. Proc.,' vol. 70, p. 502.

† 'Comptes Rendus,' vol. 135, No. 8, 25th August, 1902.

‡ 'Società degli Spettroscopisti Italiani,' vol. 1, 1872; vol. 29, 1900.

In the curves accompanying the present communication (Plate 4) the above-mentioned set, except those for 80°—90° north and south, have been grouped in pairs, thus representing the percentage frequency of prominences in each hemisphere for zones of 20° of latitude, 0°—20°, 20°—40°, &c., since it was found that this reduction could be made without losing any of the characteristic variations.

An examination of these curves shows that they differ very considerably one from the other as we proceed from the equatorial to the polar zones. Generally speaking the curves representing the variations for each of the zones, 0°—20° north and south, conform with the sun-spot curve; that is, the maxima and minima occur at about the epochs of sun-spot maxima and minima. Those for the two zones, 20°—40°, in both hemispheres conform also in the main to the general sun-spot curve, but in addition they display subsidiary maxima or changes of curvature superimposed on the main curve.

The curves for the two zones, 40°—60° north and south, have, on the other hand, hardly any likeness to the sun-spot curve, but are made up of a series of prominent maxima representing special outbursts of prominence activity.

Passing to the curves corresponding to the next zones, *i.e.*, 60°—80° north and south, these indicate two prominent outbursts lasting for a short period, showing that this region of the sun is, as a rule, practically free from prominence activity; in the remaining zones, 80°—90° north and south, the variation is small, and is a faint echo of the condition of affairs in the neighbouring zone 60°—80°.

The Magnetic Curves.

The data regarding the magnetic phenomena employed in this comparison are those brought together by Mr. William Ellis, in two published papers on magnetic phenomena.*

We may take the opportunity here of thanking Mr. Ellis for kindly communicating to us a continuation of the data published in these two papers, which information he has brought down to the year 1899.

Two classes of magnetic phenomena were there dealt with, namely, the variations from year to year of the diurnal range of the declination and horizontal force, and magnetic disturbances.

As regards the former, Mr. Ellis has shown† that the curves indicating these variations are very similar to that of the general sun-spot curve; in fact, the curves were found to be almost identical in all their smaller irregularities.

* 'Phil. Trans.,' Part II, 1880, "On the Relation between the Diurnal Range of Magnetic Declination and Horizontal Force, as observed at the Royal Observatory, Greenwich, during the years 1841 to 1877, and the Period of Solar Spot Frequency"; 'Monthly Notices, R.A.S.,' December, 1899, vol. 60, No. 2, "On the Relation between Magnetic Disturbance and the Period of Solar Spot Frequency."

† 'Phil. Trans.,' Part II, 1880.

The second class of phenomena, namely, the magnetic disturbances, which are more irregular in occurrence, has been classified by Mr. Ellis into five groups, and tabulated by him under five separate sub-heads. For the present paper, reference will only be made to one of these classes, namely, that described as "great," this group representing the largest disturbances. The curve representing the variation in number of these disturbances indicates short intermittent crests, outbursts, in fact, with rapid rises to maxima and falls to minima, and comparatively long intervals of quiescence.

Comparison of the Curves representing Prominence Frequency and Variation of Diurnal Magnetic Range.

Mr. Ellis, as already has been pointed out, has indicated the close resemblance between the sun-spot curve and that representing the variation of the magnetic elements; and it has been shown in the earlier part of this paper, that the curves representing the percentage frequency of prominences near the solar equator, conform in the main to the general sun-spot curve.

There is therefore an apparent connection between phenomena occurring in the equatorial regions of the sun (as represented by zones of prominences near the equator, and sun-spots which are practically restricted to these zones), and the ordinary diurnal magnetic variation.

The accompanying set of curves (Plate 5) illustrates the great similarity between those showing the frequency of prominences in a zone about the equator (0°—20° north and south) and the variations of the mean daily range of magnetic declination; for the sake of comparison, three other curves are added, showing the variation of the mean daily area of sun-spots for the whole, and the two hemispheres of the sun separately.*

* In referring to the curve representing the variation of the mean daily areas of sun-spots, it may be noted that this is obtained by combining the mean daily areas of both hemispheres of the sun. A closer analysis shows, however, that this variation is not the same for both hemispheres. From the year 1862, when such a division of the sun's disc can be easily investigated, the northern hemisphere, about the time of the two last maxima, displayed double maxima occurring in the years 1881 and 1884, and in the years 1892 and 1895. About the time of the maximum of 1870 this duplicity is not so marked, although when compared with the curve for the southern hemisphere for this period, there is a slight indication of a subsidiary crest in 1872. In the case of the curve representing the mean spotted area for the southern hemisphere alone, at all the three epochs of maximum, the curves are single-crested and indicate sharply-defined maxima in the years 1870, 1883, and 1893.

From the above it will be seen, therefore, that the actual epochs of sun-spot maxima, as determined from the northern and southern hemispheres respectively

Comparison of the Prominences with the Magnetic Disturbance Curves.

If a comparison of the curve representing the number of days of the "great" magnetic disturbance is made with those representing prominence frequency (Plate 4), it will be seen that the former is as unlike the curves representing the prominence frequency about the solar equator as it is like those near the poles; in fact, the polar prominence outbursts, and great magnetic disturbances occur almost simultaneously.

The peculiar form and general similarity of the curves can be best seen from the accompanying illustration (fig. 1). In the figure comparison is made between the epochs of the crossing of the known and unknown lines, the percentage frequency of prominences about the solar poles and Ellis' "great" magnetic disturbances.

Two curves representative of prominence frequency are given, one to indicate the abrupt nature of the curves representing the frequency in a zone near the pole 10 degrees in width (in this case 60°—70° north), and the second to illustrate polar action as a whole; this latter was obtained by making a summation of prominence frequency for the two zones 60°—90° north and south.

The simultaneous occurrence of the maxima suggests that, when the prominence action takes place at the polar regions of the sun, one effect on the earth is that we experience our greatest magnetic disturbances.

Further, according to Mr. Ellis,* "unusual magnetic disturbance is frequent about epochs of sun-spot maximum, and nearly or quite absent about epochs of sun-spot minimum."

We find that not only do these "great" disturbances occur at the same time as the polar prominences, but the spectroscopic observations of sun-spots show that they take place not only "about" the times of spot maximum, as stated by Mr. Ellis, but when the sun-spot curve is *approaching* a maximum and at the dates of the widened line crossings,† when the curve representing the "unknown" lines is on the rise, and crosses the "known" line which is descending. At the other epoch of "crossing," *i.e.*, when the curve showing the "known" lines is on the rise and the "unknown" is falling, there is practically no magnetic disturbance recorded. Attention is again drawn to these crossings, as it is desired to indicate that it is only at those particular times when the sun is increasing his temperature that these disturbances occur.

are not the same, and in dealing with the curve representing this variation for the whole hemisphere, this fact should be borne in mind.

It may further be noted that the epochs of minima may be practically considered the same for both hemispheres.

* 'Monthly Notices R.A.S.,' vol. 60, p. 148.

† 'Roy. Soc. Proc.,' vol. 67, p. 412.

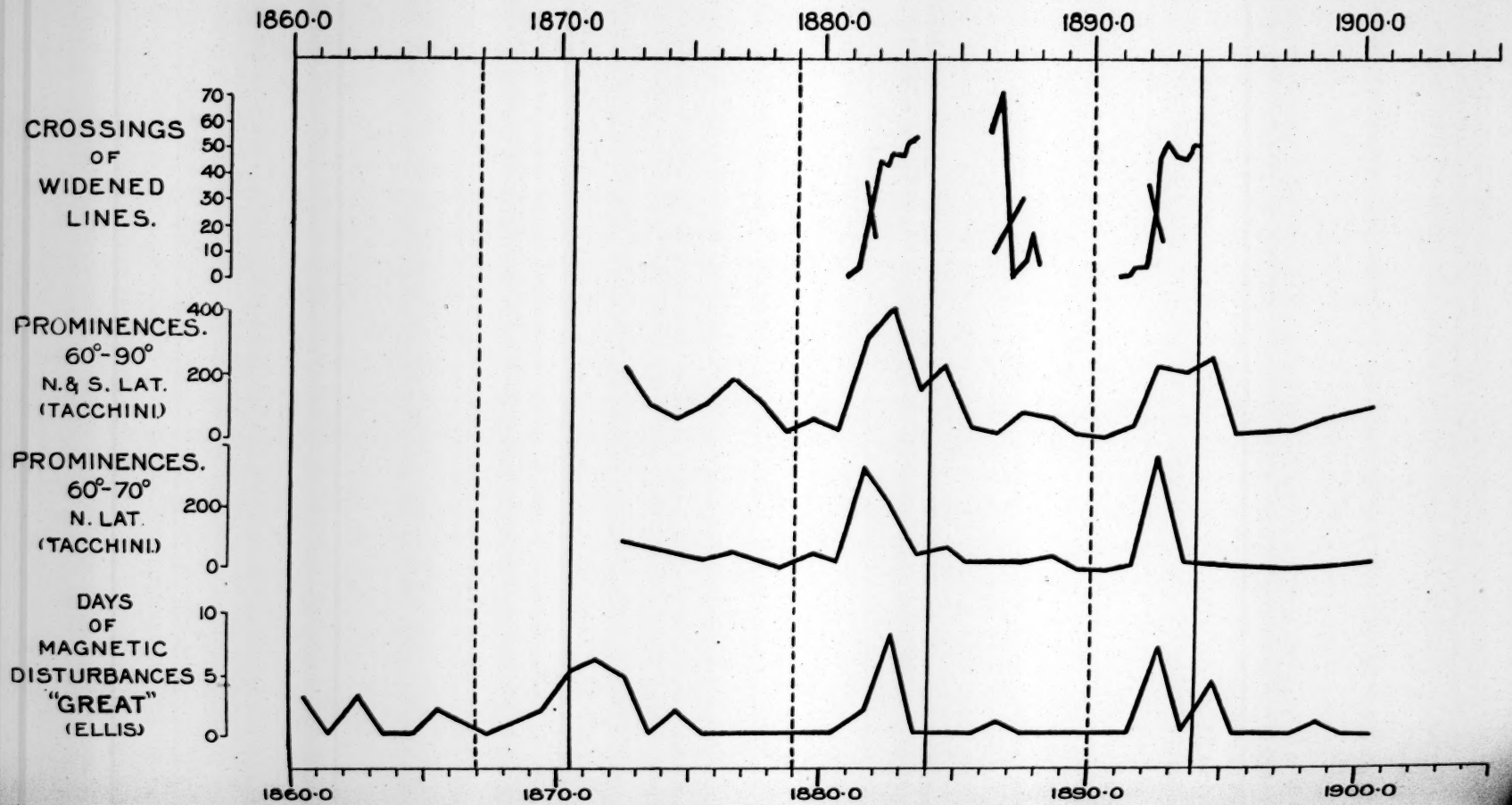


FIG. 1.—Comparison showing days of "great" magnetic disturbance, polar prominences, and crossings of widened lines. (The continuous and broken vertical lines indicate the epochs of sun-spot maxima and minima respectively.)

The facts in this paper explain why it is that magnetic storms sometimes take place when there are no spots, or no very large spots, on the surface of the sun. Since the occurrence of magnetic storms is

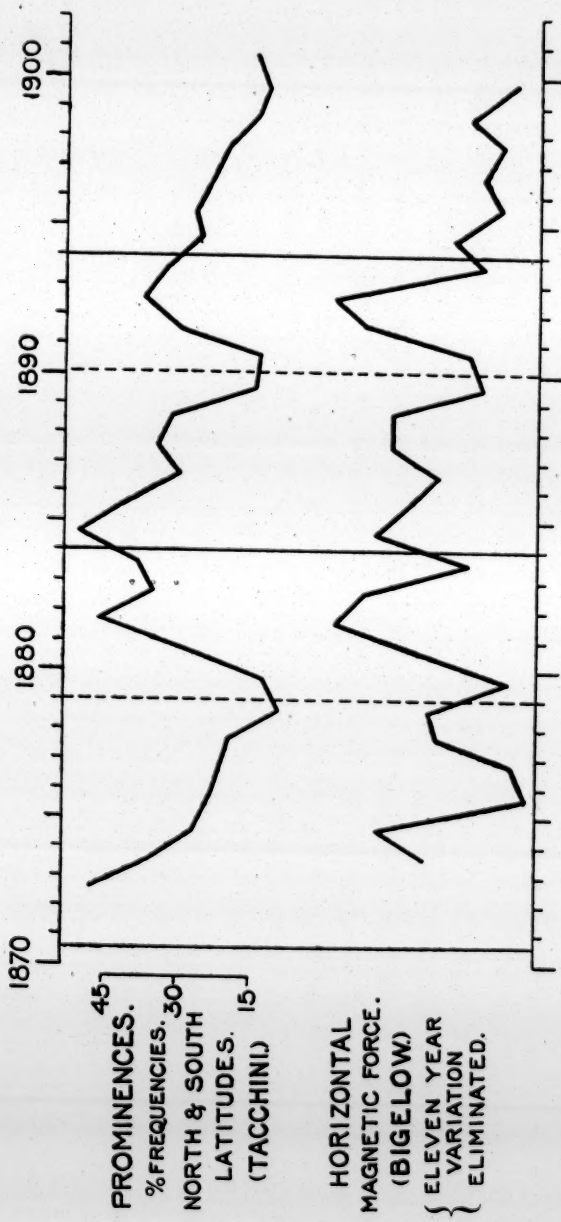


FIG. 2.—Comparison between horizontal magnetic force (Bigelow) and solar prominences (Tacchini). (Continuous and broken vertical lines as in fig. 1.)

shown to be very closely connected with the solar prominences, there may be prominences and magnetic storms when there are no spots. Prominences may also sometimes be associated with large spots, and as the latter can be seen while the former can not, the resulting magnetic storm is generally attributed to the spots.

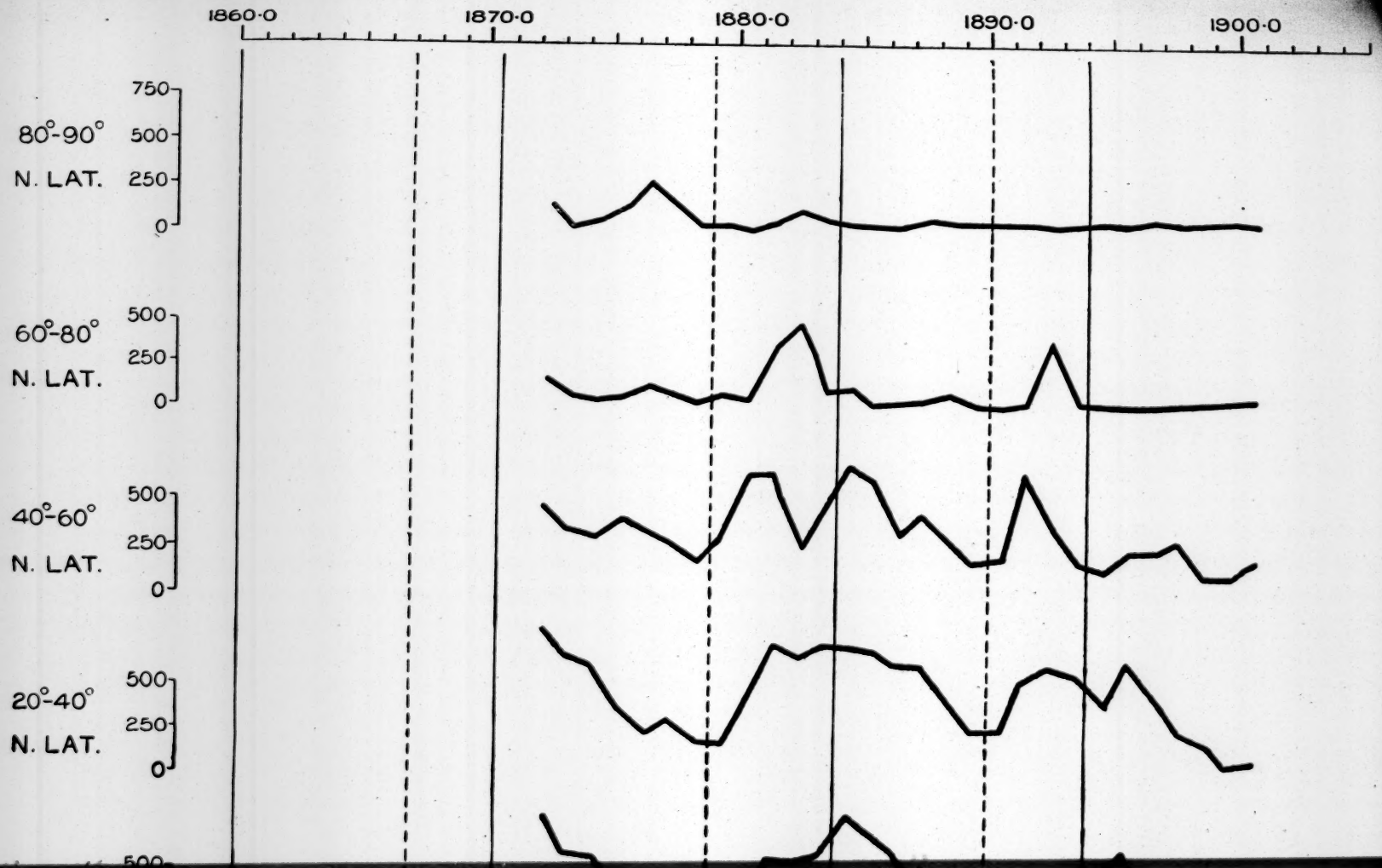
Further, the magnitude of magnetic storms appears to vary according to the particular position as to latitude of the prominence on the sun's disc. The nearer the poles (either north or south) the prominence occurs, the greater the magnetic storm, and these are the regions where no spots exist.

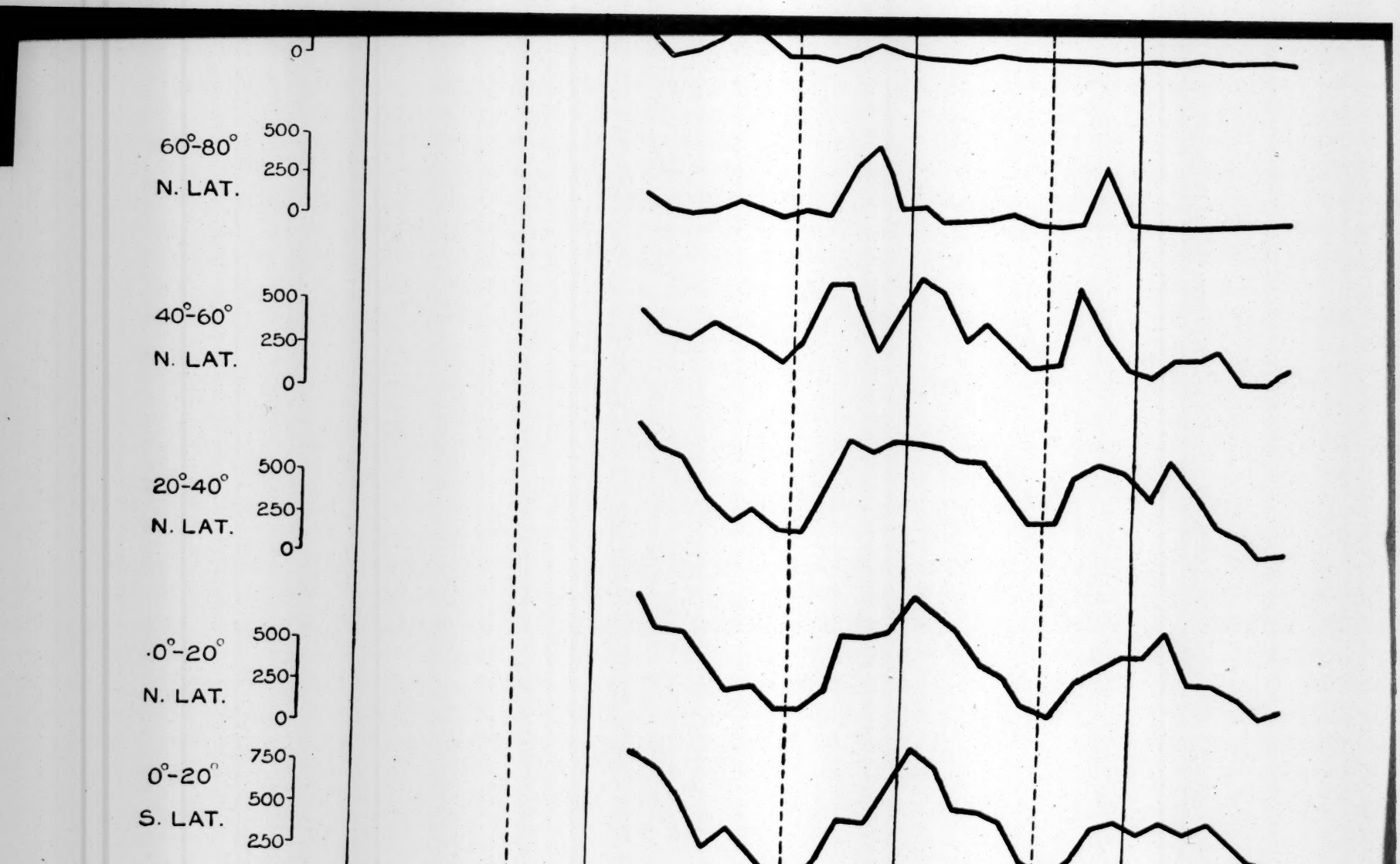
In this paper we have shown that the variations of the general magnetic phenomena, as given by Ellis, synchronise with the occurrence of prominences about the solar equator, while his "great" magnetic disturbances occur, in point of time, with the appearance of prominences in the polar regions of the sun.

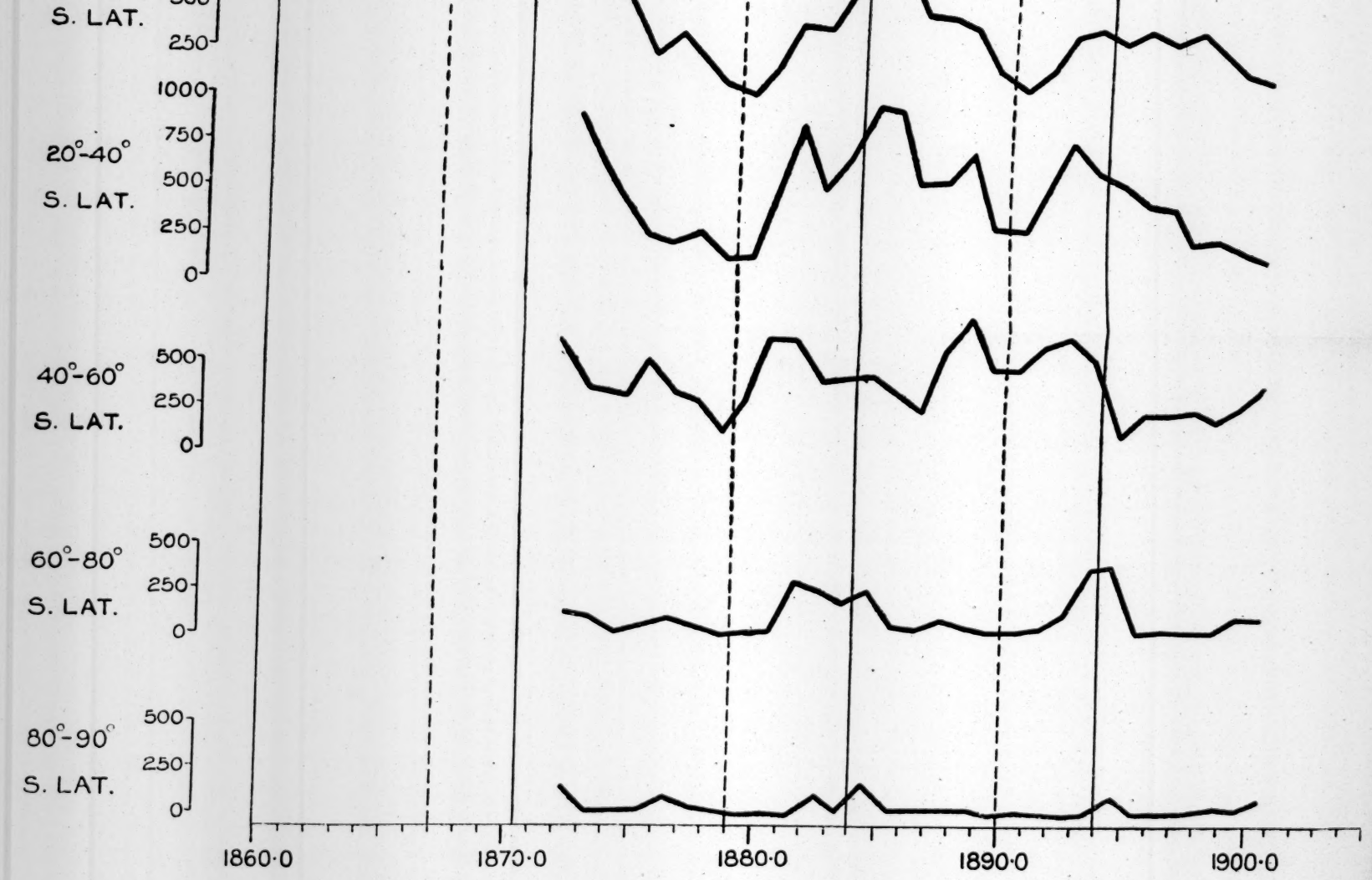
Professor Bigelow has recently* investigated the variations in the horizontal magnetic force, and finds that the curve representing these changes exhibits subsidiary maxima which synchronise with those recorded in the curve representing the mean variation of prominences for all latitudes. Thus, to use his own words, "the remarkable synchronism between the curves cannot escape recognition, except after the year 1894, when an extra minor crest is developed in the horizontal force."

The accompanying diagram (fig. 2) gives Professor Bigelow's curve, which represents, as he says, "the series of minor variations which were found in the horizontal magnetic force . . . . after the 11-year cycle curve has been eliminated," together with the percentage frequency of prominences in all latitudes obtained by us from Tacchini's observations.

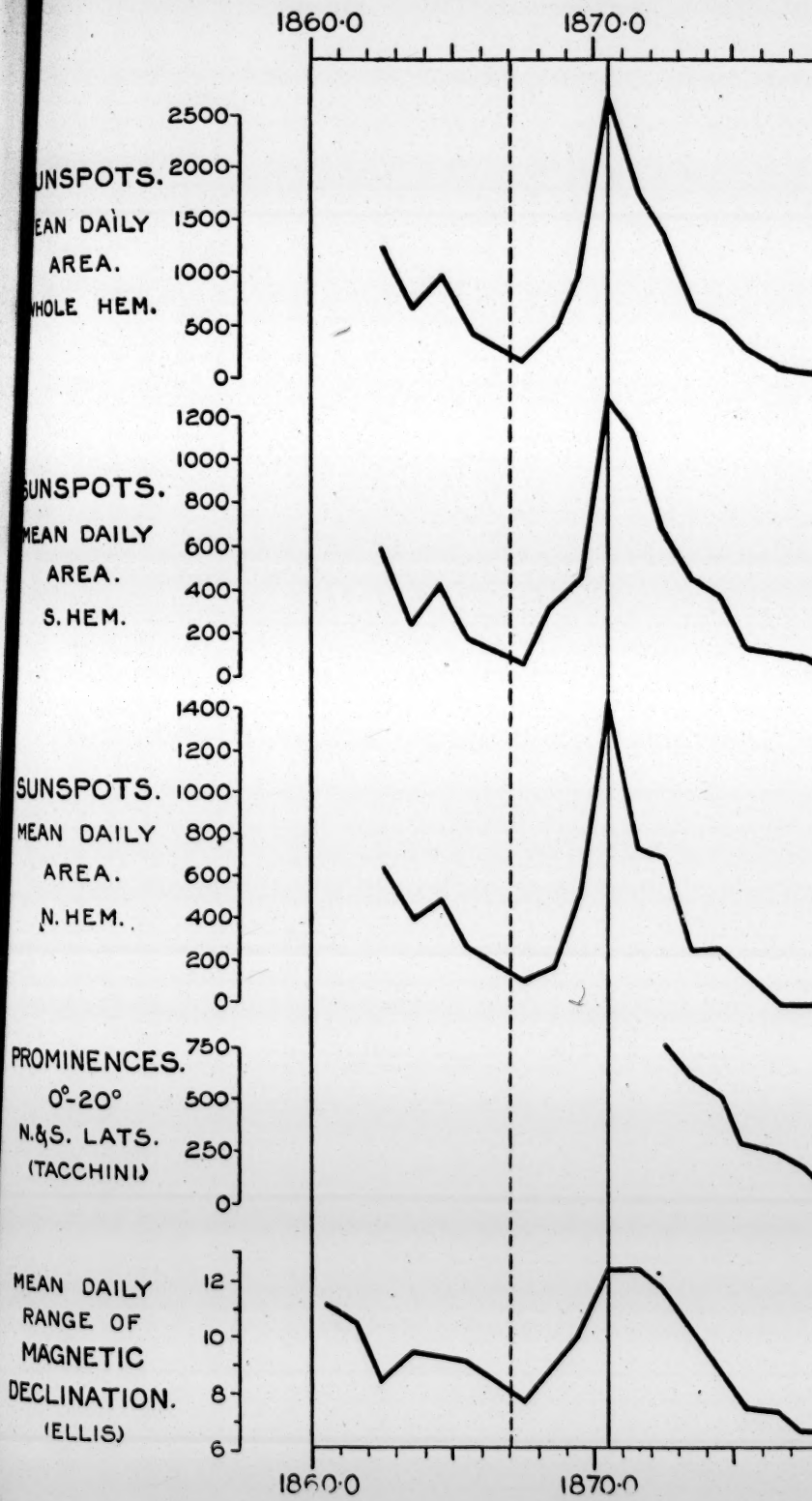
* 'Monthly Weather Review,' vol. 30, No. 7, July, 1902, p. 352.


1860·0
1870·0
1880·0
1890·0
1900·0
80°-90° N. LAT.
750
500
250
0
60°-80° N. LAT.
500
250
0
40°-60° N. LAT.
500
250
0
20°-40° N. LAT.
500
250
0
500

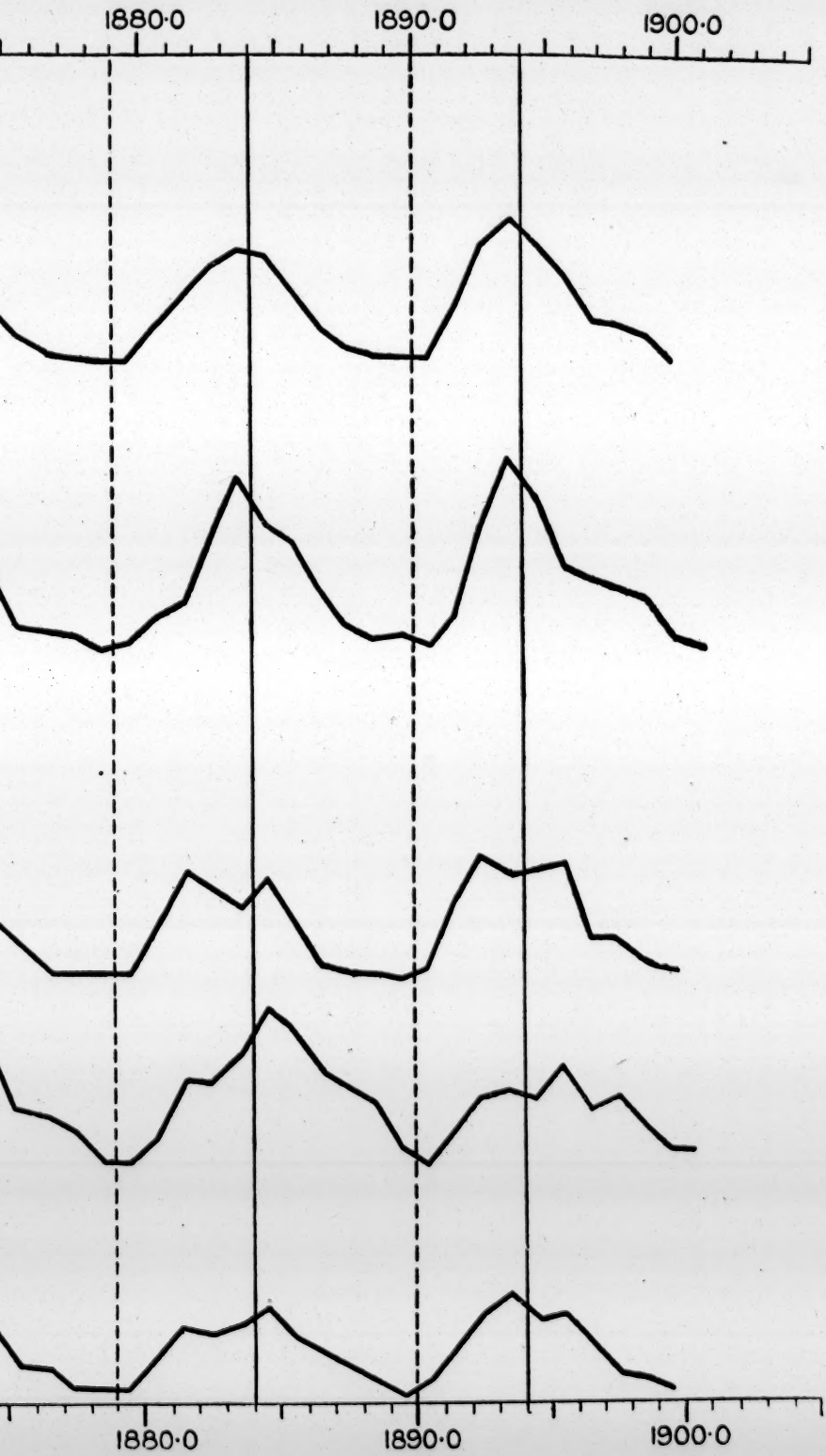

0
60°-80°
N. LAT.
500
250
0
40°-60°
N. LAT.
500
250
0
20°-40°
N. LAT.
500
250
0
0°-20°
N. LAT.
500
250
0
0°-20°
S. LAT.
750
500
250



Curves showing the percentage frequency of solar prominences for each 20° zone N. and S.
(The continuous and broken vertical lines indicate the epochs of sun-spot maxima and minima respectively.)



Comparison of curves representing variations of magnetic declinatio
(Continuous and broken verti



declination, solar prominences (0°—20° N. and S.), and sun-spot areas.
ken vertical lines as in Plate 4.)

"The Bending of Electric Waves round a Conducting Obstacle." By H. M. MACDONALD, F.R.S., Fellow of Clare College, Cambridge. Received January 21,—Read January 29, 1903.

1. The mathematical theory of the formation of a shadow when waves impinge on an obstacle rests on an application of Huygens' principle, which may be stated in the form that, if a closed surface S be drawn, enclosing all the sources of the waves, the circumstances that obtain at any point outside this surface at a definite time can be expressed in terms of the state of affairs at the surface S at previous times. For waves of sound, the usual analytical expression involves a knowledge of both the velocity potential of the motion and the velocity normal to the surface S at each point of it for all time;* for electric waves, which may be taken to include waves of light, it requires a knowledge of both the electric and magnetic forces tangential to the surface S for all time.† In the application to the theory of shadows,‡ the surface S that is chosen coincides in part with the surface of the obstacle, the remaining part being chosen so as to simplify the calculation as much as possible; for example, in the problem of the passage of waves of light through an aperture in a plane screen, the surface S is taken to be the plane of the screen. It is then assumed that the part of the surface S which coincides with the surface of the obstacle contributes nothing, and it follows that, when the wave-length is small, a shadow is formed, whose boundary is determined by the extreme incident rays that meet the surface of the obstacle. The assumption thus made is equivalent to assuming that the obstacle is perfectly absorbing, and for waves of light incident on opaque bodies this is known to be approximately true, with possible exception for the case of opaque bodies whose surfaces are polished. For waves of sound incident on an approximately rigid obstacle, and for electric waves incident on an approximately perfectly conducting body, this theory does not apply: as, in the first case, the condensation does not vanish at the surface of the obstacle; and, in the second case, the tangential magnetic force does not vanish at the surface.

In what follows the behaviour of electric waves incident on a perfectly conducting body will be discussed, and the conditions necessary for the formation of a shadow in this case will incidentally appear. The results for waves of sound incident on a rigid obstacle are very similar.

2. It will be sufficient to consider a comparatively simple case,

* Lord Rayleigh, 'Theory of Sound,' vol. 2, § 293.

† Macdonald, 'Electric Waves,' § 14.

‡ *Cf.* Stokes, 'Camb. Phil. Trans.,' vol. 9, 1849, p. 1; Lorenz, 'Pogg. Ann.,' p. 111, 1860 Kirchhoff, 'Berlin Sitzungsberichte,' vol. 2, 1882, p. 641.

which is of some importance on account of its application to the propagation of electric waves along the surface of the earth. The case is that of a Hertzian oscillator placed outside a perfectly conducting sphere. Let the radius of the sphere be a, and let the oscillator be at the point C, whose distance OC from O the centre of the sphere is r_1, the direction of the axis of the oscillator being along the line OC. The lines of magnetic force are circles, whose centres lie on OC, and whose planes are perpendicular to OC. If γ denotes the magnetic force at any point P whose distance from OC is ρ, $\gamma\rho$ satisfies the differential equation

$$\frac{\partial^2}{\partial\rho^2}(\gamma\rho) - \frac{1}{\rho}\frac{\partial}{\partial\rho}(\gamma\rho) + \frac{\partial^2}{\partial z^2}(\gamma\rho) + \kappa^2\gamma\rho = 0,$$

where z is the distance of the point P from some plane of reference perpendicular to OC, and $2\pi/\kappa$ is the wave-length of the oscillations. Transforming to polar co-ordinates (r, θ), where $r = \mathrm{OP}$ and θ is the angle COP, this equation becomes

$$\frac{\partial^2}{\partial r^2}(\gamma\rho) + \frac{1-\mu^2}{r^2}\frac{\partial^2}{\partial\mu^2}(\gamma\rho) + \kappa^2\gamma\rho = 0 \quad \ldots\ldots\ldots\ldots \quad (1),$$

in which $\mu = \cos\theta$. The general solution of this equation, which is applicable to the space external to a sphere, is

$$\gamma\rho = r^{\frac{1}{2}}\sum_1^\infty \{A_n J'_{n+\frac{1}{2}}(\kappa r) + B_n J_{-n-\frac{1}{2}}(\kappa r)\}(1-\mu^2)\frac{\partial P_n(\mu)}{\partial\mu},$$

in which $J_m(\kappa r)$ denotes Bessel's function of order m and $P_n(\mu)$ the zonal harmonic of integral order n. It is, therefore, first necessary to express the magnetic force due to the oscillator in this form. If γ_1 is the magnetic force at the point P due to the oscillator, γ_1 is the real part of $C\frac{\partial}{\partial\rho}\frac{e^{-\iota\kappa(R-Vt)}}{R}$, in which R is the distance CP and V is the velocity of radiation.* Writing

$$\psi_1 = \rho\frac{\partial}{\partial\rho}\frac{e^{-\iota\kappa R}}{R},$$

and remembering that $R^2 = r^2 + r_1^2 - 2rr_1\mu$,

this is equivalent to

$$\psi_1 = \rho\frac{\partial}{\partial\rho}r^{-\frac{1}{2}}r_1^{-\frac{1}{2}}e^{\pi\iota/4}\sum_0^\infty (2n+1)\,e^{n\pi\iota/2}K_{n+\frac{1}{2}}(\iota\kappa r_1)\,J_{n+\frac{1}{2}}(\kappa r)\,P_n(\mu),$$

when $r < r_1$, where

$$K_m(\iota\kappa r) = \frac{\pi e^{-\frac{m\pi\iota}{2}}}{2\sin m\pi}[J_{-m}(\kappa r) - e^{m\pi\iota}J_m(\kappa r)].\dagger$$

* Hertz, 'Electric Waves,' Eng. Trans., p. 141.

† Macdonald, 'Proc. Lond. Math. Soc.,' vol. 32.

Now

$$\rho\frac{\partial}{\partial\rho} = (1-\mu^2)\left[r\frac{\partial}{\partial r} - \mu\frac{\partial}{\partial\mu}\right],$$

$$(2n+1)\mathrm{P}_n = \frac{\partial \mathrm{P}_{n+1}}{\partial\mu} - \frac{\partial \mathrm{P}_{n-1}}{\partial\mu},$$

$$(2n+1)\mu\frac{\partial \mathrm{P}_n}{\partial\mu} = n\frac{\partial \mathrm{P}_{n+1}}{\partial\mu} + (n+1)\frac{\partial \mathrm{P}_{n-1}}{\partial\mu};$$

making these substitutions and rearranging the series, it becomes

$$\psi_1 = r_1^{-\frac{1}{2}}e^{\pi\iota/4}\sum_1^\infty\left[e^{(n-1)\frac{\pi\iota}{2}}\mathrm{K}_{n-\frac{1}{2}}(\iota\kappa r_1)\left\{r\frac{\partial}{\partial r}r^{-\frac{1}{2}}\mathrm{J}_{n-\frac{1}{2}}(\kappa r)\right.\right.$$

$$\left. - (n-1)r^{-\frac{1}{2}}\mathrm{J}_{n-\frac{1}{2}}(\kappa r)\right\} - e^{(n+1)\frac{\pi\iota}{2}}\mathrm{K}_{n+\frac{3}{2}}(\iota\kappa r_1)\left\{r\frac{\partial}{\partial r}r^{-\frac{1}{2}}\mathrm{J}_{n+\frac{3}{2}}(\kappa r)\right.$$

$$\left.\left. + (n+2)r^{-\frac{1}{2}}\mathrm{J}_{n+\frac{3}{2}}(\kappa r)\right\}\right](1-\mu^2)\frac{\partial \mathrm{P}_n}{\partial\mu},$$

that is

$$\psi_1 = r^{\frac{1}{2}}\sum_1 g_n(r_1)\mathrm{J}_{n+\frac{1}{2}}(\kappa r)(1-\mu^2)\frac{\partial \mathrm{P}_n}{\partial\mu},\ r<r_1 \quad \ldots\ldots\ldots\ldots \quad (2),$$

where

$$g_n(r_1) = -\kappa r_1^{-\frac{1}{2}}e^{\pi\iota/4}\left\{e^{(n-1)\frac{\pi\iota}{2}}\mathrm{K}_{n-\frac{1}{2}}(\iota\kappa r_1) + e^{(n+1)\frac{\pi\iota}{2}}\mathrm{K}_{n+\frac{3}{2}}(\iota\kappa r_1)\right\}.$$

If, then, a solution ψ of equation (1) can be found, which is such that ψ becomes infinite as ψ_1 at the point $(r_1, 0)$ and $\frac{\partial\psi}{\partial r}$ vanishes when $r = a$, the real part of $\mathrm{C}\psi e^{\iota\kappa\mathrm{V}t}$ will be γ, the required magnetic force, for then $\frac{\partial}{\partial r}(\gamma\rho)$ will vanish when $r = a$, that is, the electric force tangential to the sphere vanishes. The solution required will be of the form

$$\psi = r^{\frac{1}{2}}\sum_1^\infty g_n(r_1)\left[\mathrm{J}_{n+\frac{1}{2}}(\kappa r) + \mathrm{A}_n\mathrm{K}_{n+\frac{1}{2}}(\iota\kappa r)\right](1-\mu^2)\frac{\partial \mathrm{P}_n}{\partial\mu},$$

where $r_1 > r > a$, and the constants A_n are determined by the condition that $\frac{\partial\psi}{\partial r} = 0$, when $r = a$; hence

$$\psi = r^{\frac{1}{2}}\sum_1^\infty g_n(r_1)\left[\mathrm{J}_{n+\frac{1}{2}}(\kappa r) - \frac{\frac{\partial}{\partial a}\{a^{\frac{1}{2}}\mathrm{J}_{n+\frac{1}{2}}(\kappa a)\}}{\frac{\partial}{\partial a}\{a^{\frac{1}{2}}\mathrm{K}_{n+\frac{1}{2}}(\iota\kappa a)\}}\mathrm{K}_{n+\frac{1}{2}}(\iota\kappa r)\right](1-\mu^2)\frac{\partial \mathrm{P}_n}{\partial\mu},$$

where $r_1 > r > a$.

The calculation of the electric forces at a point not on the surface of the sphere presents difficulties, but when the wave-length is small compared with the radius of the sphere, the electric force at the surface of the sphere can be obtained in a simple form. The electric force at the surface of the sphere is normal to the surface, and denoting it by F,

$\frac{1}{V^2}\frac{\partial F}{\partial t}$ is the real part of $\frac{C}{a^2}\frac{\partial \psi}{\partial \mu}$ when $r = a$. Now

$$\frac{1}{a^2}\frac{\partial \psi}{\partial \mu_{r=a}} = a^{-\frac{3}{2}}\frac{\partial}{\partial \mu}\sum_1^\infty g_n(r_1)\left[J_{n+\frac{1}{2}}(\kappa a) - \frac{K_{n+\frac{1}{2}}(\iota\kappa a)}{\frac{\partial}{\partial a}\{a^{\frac{1}{2}}K_{n+\frac{1}{2}}(\iota\kappa a)\}}\frac{\partial}{\partial a}\{a^{\frac{1}{2}}J_{n+\frac{1}{2}}(\kappa a)\}\right](1-\mu^2)\frac{\partial P_n}{\partial \mu},$$

and, when the wave-length is small compared with a, this becomes

$$\frac{1}{a^2}\frac{\partial \psi}{\partial \mu_{r=a}} = a^{-\frac{3}{2}}\frac{\partial}{\partial \mu}\sum_1^\infty g_n(r_1)\left[J_{n+\frac{1}{2}}(\kappa a) + \frac{a^{-\frac{1}{2}}}{\iota\kappa}\frac{\partial}{\partial a}\{a^{\frac{1}{2}}J^{n+\frac{1}{2}}(\kappa a)\}\right](1-\mu^2)\frac{\partial P_n}{\partial \mu},$$

that is, writing $\psi_1 = f(r)$,

$$\frac{1}{a^2}\frac{\partial \psi}{\partial \mu_{r=a}} = \frac{1}{a^2}\frac{\partial}{\partial \mu}\left[f(a) + \frac{1}{\iota\kappa}\frac{\partial}{\partial a}\{f(a)\}\right].$$

From the above

$$f(a) = (1-\mu^2)\left(a\frac{\partial}{\partial a} - \mu\frac{\partial}{\partial \mu}\right)\frac{e^{-\iota\kappa R_0}}{R_0},$$

where $R_0^2 = a^2 + r_1^2 - 2ar_1\mu$, that is

$$f(a) = (1-\mu^2)\left(a\frac{\partial R_0}{\partial a} - \mu\frac{\partial R_0}{\partial \mu}\right)\frac{\partial}{\partial R_0}\frac{e^{-\iota\kappa R_0}}{R_0},$$

or,

$$f(a) = \frac{a^2(1-\mu^2)}{R_0}\frac{\partial}{\partial R_0}\frac{e^{-\iota\kappa R_0}}{R_0};$$

therefore

$$\frac{1}{a^2}\frac{\partial \psi}{\partial \mu_{r=a}} = \frac{1}{a^2}\frac{\partial}{\partial \mu}\left[(1-\mu^2)\left\{\frac{a^2}{R_0}\frac{\partial}{\partial R_0}\frac{e^{-\iota\kappa R_0}}{R_0} + \frac{1}{\iota\kappa}\frac{\partial}{\partial a}\frac{a^2}{R_0}\frac{\partial}{\partial R_0}\frac{e^{-\iota\kappa R_0}}{R_0}\right\}\right],$$

which gives

$$\frac{1}{V^2}\frac{\partial F}{\partial t} = \frac{C}{a^2}\frac{\partial}{\partial \mu}\left[(1-\mu^2)\frac{a^2}{R_0}\frac{\partial}{\partial R_0}\frac{\cos\kappa(R_0 - Vt)}{R_0} - \frac{1}{\kappa}\frac{\partial}{\partial a}\frac{a^2}{R_0}\frac{\partial}{\partial R_0}\frac{\sin\kappa(R_0 - Vt)}{R_0}\right\}\right],$$

and therefore

$$F = -\frac{CV}{\kappa a^2}\frac{\partial}{\partial\mu}\left[(1-\mu^2)\frac{a^2}{R_0}\frac{\partial}{\partial R_0}\frac{\sin\kappa(R_0-Vt)}{R_0} + \frac{1}{\kappa}\frac{\partial}{\partial a}\frac{a^2}{R_0}\frac{\partial}{\partial R_0}\frac{\cos\kappa(R_0-Vt)}{R_0}\right\}\right] \quad \ldots\ldots\ldots\ldots(3).$$

At points on the surface of the sphere for which R_0 is great compared with the wave-length, this becomes, retaining only the most important terms,

$$F = -CV\frac{\kappa a r_1}{R_0^3}\left(1-\frac{a-r_1\mu}{R_0}\right)(1-\mu^2)\sin\kappa(R_0-Vt),$$

or

$$F = \left(1-\frac{a-r_1\mu}{R_0}\right)F_1 = (1-\cos\chi)F_1 \quad \ldots\ldots\ldots\ldots(4),$$

where χ is the angle subtended by OC, and F_1 is the electric force along the normal, which would be due to the oscillator if the sphere were absent. From this it follows that the ratio F/F_1 gradually diminishes as μ decreases, until μ approaches the value -1, when it becomes comparable with the terms which have been neglected. Hence, when electric waves are incident on a perfectly conducting sphere, there is no true shadow near the surface when the wave-length is small compared with the radius of the sphere. It can, therefore, be inferred that, when electric waves are incident on a perfectly conducting body whose surface is convex, and has its radii of curvature everywhere great compared with the wave-length, there is no true shadow near to the surface. It is known that, when electric waves of small wave-length are incident on a perfectly conducting wedge, the disturbance does not sensibly creep round the corner, but shoots out so that there is a shadow which coincides the more closely with the geometrical shadow as the wave-length diminishes.* It therefore appears that the condition for the formation of a distinct shadow near the surface of a perfectly conducting body, whose surface is convex, when electric waves are incident on it, is that there should be a line on that part of the surface inside the geometrical shadow along which the radius of curvature of the surface in the plane of incidence of the waves is small compared with the wave-length.

3. The electric force normal to the surface of a sphere, which is a fairly good conductor, may be obtained by an analysis similar to that given above. The result is

$$\frac{1}{a^2}\frac{\partial\psi}{\partial\mu_{r=a}} = \frac{1}{a^2}\frac{1-\iota\epsilon}{1+\epsilon^2}\frac{\partial}{\partial\mu}\left[f(a)+\frac{1}{\iota\kappa}\frac{\partial}{\partial a}f(a)\right],$$

* Sommerfeld, 'Math. Annalen,' vol. 47, 1896; or Macdonald, 'Electric Waves,' 1902, p. 187.

where $\epsilon = \sigma\kappa/4\pi V$, and $\gamma_1\rho$ is the real part of $f(r)$, and σ is the specific resistance of the material of the sphere, the other symbols having the same meaning as before; it being assumed that ϵ, which for ordinary metallic conductors is of the order 10^{-7} when the wave-length is about 10 cm., is small. If F′ now denotes the electric force normal to the surface, F′ differs in phase from F (the electric force normal to the surface when the sphere is a perfect conductor) by a small amount, and the ratio of the amplitude of F′ to F is $1 - \frac{1}{2}\epsilon^2$. The effect of imperfect conduction is therefore to diminish the electric force normal to the surface, but only by an inappreciable amount when the obstacle is as good a conductor as an ordinary metal; for sea water, taking $\sigma = 10^{10}$, the correction is less than one part in a thousand.*

4. The effect of a rigid spherical obstacle on the waves of sound sent out from a source, when the wave-length is small compared with the radius of the sphere, can be obtained by an analysis which s almost identical with that given above. The result is that at any point on the sphere at a distance from the source great compared with the wave-length,

$$\phi = \phi_1\left(1 - \frac{a - r_1\mu}{R_0}\right) = \phi_1(1 - \cos\chi),$$

where ϕ_1 is the velocity potential at the point due to the source, and ϕ is the actual velocity potential there. There is, therefore, no true shadow near the surface of the sphere. Lord Rayleigh† has discussed the effect of a rigid sphere on the waves sent out from a source close to the surface, and found that there was no indication of the formation of a shadow for wave-lengths greater than half the circumference. The above statement completes the investigation. The conditions for the formation of a distinct shadow, when waves of sound are incident on an approximately rigid obstacle, follow; they are of the same type as those already stated for an approximately perfectly conducting body on which electric waves are incident.

5. The results of § 2 have an immediate application to the question of the propagation of electric waves around the surface of the earth. Let C be a place on the earth's surface from which waves are being sent out; these waves may be supposed to be due to an oscillator placed vertically. The electric force acting on a receiver at a place, whose angular distance from C measured along a great circle is θ, will be F, given by equation (4) § 2, when the distance of the receiver from the oscillator is great compared with the wave-length. In this case r_1 is nearly equal to a, and may be put equal to it for values of θ for which equation (4) applies; thus $F = F_1(1 - \sin\frac{1}{2}\theta)$. It is

* For the wave-lengths actually used the correction is less than one part in a hundred millions.

† 'Theory of Sound,' vol. 2, § 328.

convenient for purposes of comparison to substitute for F_1 in terms of another quantity; let F_2 denote the electric force due to the oscillator at a point in its equatorial plane at a distance $a\theta$ from the oscillator, which is the same as the arcual distance of the receiver; then

$$\frac{\overline{F}}{\overline{F_2}} = \frac{\theta \cos^2 \frac{1}{2}\theta (1 - \sin \frac{1}{2}\theta)}{2 \sin \frac{1}{2}\theta} = k,$$

where $\overline{F}$ is the amplitude of F, and $\overline{F_2}$ the amplitude of F_2; the ratio of the intensities in the two cases is k^2. The following table shows the manner of variation of the amplitudes and the intensity (k^2) near the sphere as θ increases from 20° to 120°:—

θ	$1-\sin\frac{\theta}{2}$	k	k^2
20°	0·82635	0·80551	0·64885
25°	0·78356	0·73567	0·54121
30°	0·74118	0·69949	0·48929
35°	0·69929	0·64546	0·41661
40°	0·65797	0·59297	0·35162
45°	0·61731	0·54070	0·29235
50°	0·57738	0·48964	0·23975
55°	0·53825	0·44019	0·19377
60°	0·5	0·39269	0·15421
65°	0·46270	0·34745	0·12072
70°	0·42642	0·30473	0·09286
75°	0·39127	0·26477	0·07010
80°	0·35721	0·22766	0·05183
85°	0·32440	0·19343	0·03741
90°	0·29289	0·16266	0·02645
95°	0·26272	0·13483	0·01818
100°	0·23395	0·11011	0·01212
105°	0·20664	0·08844	0·00782
110°	0·18084	0·06972	0·00486
115°	0·15660	0·05379	0·00289
120°	0·13397	0·04050	0·00164

For example, when $\theta = \frac{1}{4}\pi$, that is for the case of the earth at a distance of rather more than 3000 miles, the amplitude of the electric force acting on the receiver is more than half the amplitude of the electric force that would be directly due to the oscillator at that distance, and the intensity nearly three-tenths. These results will apply when the two places are separated by good conducting material such as sea water, the effect of the imperfect conduction of such substances being by § 3 negligible. They explain why wireless telegraphy is more effective over the sea or wet soil than over dry soil; from § 3 it follows that a badly-conducting obstacle diminishes the effect. It is also to be expected from § 2 that the influence of a

ridge of some sharpness between the places is to create a distinct shadow, to such an extent that the effect would be inappreciable; the same result would be produced by an intervening headland; this agrees with the experience of Captain Jackson.*

"Studies in the Morphology of Spore-producing Members.—No. V. General Comparisons, and Conclusion." By F. O. Bower, Sc.D., F.R.S., Regius Professor of Botany in the University of Glasgow. Received January 30,—Read February 12, 1903.

(Abstract.)

This concluding Memoir contains a general discussion of the results acquired in the four previous parts of this series, and of their bearing on a theory of sterilisation in the sporophyte. The attempt is made to build up the comparative morphology of the sporophyte from below, by the study of its simpler types; the higher and more specialised types are left out of account, except for occasional comparison. It is assumed for the purposes of the discussion that alternation of generations in the Archegoniatæ is of the antithetic type, and that apogamy and apospory are abnormalities, not of primary origin.

After a brief allusion to facts of sterilisation in the Sporogonia of Bryophytes, the similar facts are summarised for the Pteridophytes. It has been found that examples of sterilisation of potentially sporogenous cells are common also in vascular plants, while occasionally cells which are normally sterile may develop spores. Hence it is concluded that spore-production in the Archegoniate plants is not in all cases strictly limited to, or defined by, preordained formative cells, or cell-groups. A discussion of the archesporium follows, and though it is found that in all Pteridophyta the sporogenous tissue is ultimately referable to the segmentation of a superficial cell, or cells, still in them, and indeed in vascular plants at large, the segmentations which lead up to the formation of spore-mother-cells are not comparable in all cases; in fact, that there is no general law of segmentation underlying the existence of that cell or cells which a last analysis may mark out as the "archesporium"; nor do these ultimate parent cells give rise in all cases to cognate products. Therefore it is concluded that the general application of a definite term to those ultimate parent cells which the analysis discloses has no scientific meaning, beyond the statement of the histiogenic fact.

Further, it is shown that the tapetum is not a morphological constant,

* 'Roy. Soc. Proc.,' 1902.

but varies both in occurrence and origin; that even the individuality of the sporangium is not always maintained. All that remains then as the fundamental conception of the sporangium in vascular plants is the spore-mother-cell, or cells, and the tissue which covers them in, for such cells are always produced internally. The definition of the sporangium may then be given thus: "*Wherever we find in vascular plants a single spore-mother-cell, or connected group of them, or their products, this with its protective tissues constitutes the essential of an individual sporangium.*" From the point of view of a theory of sterilisation such sporangia may, at least in the simplest cases, be regarded as islands of fertile tissue which have retained their spore-producing character, while the surrounding tissues have been diverted to other uses. It will be seen later how far this view will have to be modified in the more complex cases.

In a second section of the Memoir the variations in number of sporangia in vascular plants are discussed; the methods of variation may be tabulated as follows, under the heads of progressive increase and decrease:—

I.—*Increase in Number of Sporangia.*

(*a.*) By septation, with or without rounding off of the individual sporangia.
(*b.*) By formation of new sporangia, or of new spore-bearing organs, which may be in addition to, or interpolated between those typically present.
(*c.*) By continued apical, or intercalary growth of the parts bearing the sporangia.
(*d.*) By branching of the parts bearing the sporangia.
(*e.*) Indirectly, by branchings in the non-sporangial region resulting in an increased number of sporangial shoots; this is closely related to (*c*) and (*d*).

II.—*Decrease in Number of Sporangia.*

(*f.*) By fusion of sporangia originally separate.
(*g.*) By abortion, partial or complete, of sporangia.
(*h.*) By reduction or arrest of apical or intercalary growth in parts bearing sporangia.
(*i.*) By fusion of parts which bear the sporangia or arrest of their branchings.
(*j.*) Indirectly, by suppression of branchings in the non-sporangial region, resulting in decreased number of sporangial shoots; this is closely related to (*h*) and (*i*).

We are justified in assuming that (subject to the possibility of other factors having been operative of which we are yet unaware)

the condition of any polysporangiate sporophyte as we see it is the resultant of modifications such as these, operative during its descent.

The problem will, therefore, be in each case to assign its proper place in the history to any or each of these factors.

It is pointed out that in homosporous types, which are certainly the more primitive, the larger the number of spores the better the chance of survival, and hence, other things being equal, increasing numbers of spores and of sporangia may be anticipated; but in the heterosporous types reduction in number both of spores and of sporangia is frequent. The former will accordingly illustrate more faithfully than the heterosporous forms the story of the increase of complexity of spore-producing parts. The general method put in practice here is to regard homosporous forms as in the upgrade of their evolution, as regards their spore-producing organs, unless there is clear evidence to the contrary. The *onus probandi* lies rather with those who assume reduction to have taken place in them.

A summary of evidence of variation in number of sporangia by any of these methods is then given for the Lycopodineæ, Psilotaceæ, Sphenophylleæ, Ophioglossaceæ, Equisetineæ, and Filicineæ; followed in each case by a theoretical discussion of the bearing of that evidence on the morphology of the spore-producing members. The general result is that all of them, including even the dorsiventral and megaphyllous types, are referable to modifications of a radial strobiloid type; progressive elaboration of spore-producing parts, followed by progressive sterilisation, and especially by abortion of sporangia in them, of which there is frequent evidence, together with the acquirement of a dorsiventral structure, may be held to account for the origin of even the most complex forms. But the vegetative organs once formed may also undergo elaboration, and differentiation *pari passu* with the spore-producing organs, a point which has greatly complicated the problem, especially in the higher forms; all roots are probably of secondary origin; facts of interpolation of additional sporangia, especially in Ferns, and of apogamy and apospory, are also disturbing influences, which have probably been of relatively recent acquisition.

A comparison is drawn as regards position, physiological and evolutionary, in the sporophyte between the fertile zone in certain Bryophytes and the fertile region of certain simple Pteridophytes, *e.g.*, the Lycopods; though no community of descent is assumed, the relation of the reproductive to the vegetative regions is the same. In the Bryophytes that region is regarded as a residuum from progressive sterilisation; it is suggested that the same is the case for a strobiloid Pteridophyte, such as *Lycopodium*. The theory of the strobilus, based on this comparison, is that similiar causes would lead to

the decentralisation of the fertile tissue in the primitive Pteridophytes as in the Bryophytes, and result in the formation of a central sterile tract, with an archesporium at its periphery; that such an archesporium, instead of remaining a concrete layer as it is in the larger Musci, became discrete in the Lycopods; that the fertile cell-groups formed the centres of projecting sporangia, and that they were associated regularly with outgrowths, perhaps of correlative vegetative origin, which are the sporophylls.

Whether or not this hypothesis of the origin of a Lycopod strobilus approaches the actual truth, comparison points out the genus *Lycopodium* as a primitive one, characterised by more definite numerical and topographical relation of the sporangia to the sporophylls than in any other type of Pteridophyta.

Then follows, as a consequence of comparison, the enunciation of a theory of the sporangiophore, a word which is here used in an extended sense to include not only the spore-producing organs of Psilotaceæ, Sphenophylleæ, Ophioglossaceæ, Equisetaceæ, but also the sori of Ferns. The view is upheld that all these are simply placental growths, and not the result of "metamorphosis" of any parts or appendages of prior existence; that the vascular supply, which is not always present, is not an essential feature; that they are seated at points where in the ancestry spore-production has been proceeding on an advancing scale; hence they do not occupy any fixed and definite position. It seems probable that at least a plurality of sporangia existed on primitive sporangiophores, and that where only one exists that condition has been the result of reduction.

The above theories are then applied to the several types of Pteridophyta. The Lycopods, Psilotaceæ, Sphenophylleæ, and Ophioglossaceæ may be arranged as illustrating the increased complexity of the spore-producing parts, and of the subtending sporophylls; the factors of the advance from the simple sporangium to the more complex sporangiophore are, septation, upgrowth of the placenta with vascular supply into it, and branching, with apical growth also in the Ophioglossaceæ. But even in the most complex forms the sporangiophore may be regarded as a placental growth, and not the result of transformation of any other member.

In the case of *Helminthostachys* the marginal sporangiophores are regarded as amplifications from the sunken sporangia of the *Ophioglossum* type; in *Equisetum* they are regarded as being directly seated on the axis, and having originated there by a similar progression: they would thus be non-foliar. It is pointed out that though a foliar theory would be possible for *Equisetum* itself, it is not applicable to the facts known for the fossil Calamarieæ, which are so naturally related to it. Thus the strobilus of the Equisetineæ is of a rather different type from that of the Lycopods, Psilotaceæ, or even the

Ophioglossaceæ, in all of which there is a constant relation of the spore-producing parts to the leaves; in the Equisetineæ no such constant relation exists; the leaves and sporangiophores may be in juxtaposition, as in *Calamostachys*, without exactly matching numerically; or the sporangiophores may occur in larger numbers and in several ranks, between successive leaf-sheaths, as in *Phyllotheca* and *Bornia*; or without any leaves at all, as in *Equisetum*. Thus, on a non-phyllome theory the latter may be held to be only an extreme case of what is seen in certain fossils.

The Ferns, notwithstanding their apparent divergence of character from other Pteridophytes, may also be regarded as strobiloid forms, with greatly enlarged leaves; the primitive sori of the Simplices resemble the sporangiophores of other Pteridophytes; the more complicated soral conditions of the Gradatæ and Mixtæ were probably derivative from these, the chief difference being due to the interpolation of new sporangia, an innovation which is in accordance with biological probability, as well as with the palæontological record.

The effect of the results thus obtained on the systematic grouping of the Pteridophytes is then discussed; it is pointed out that the Lycopods, Psilotaceæ, Sphenophylleæ, Ophioglossaceæ, and Filices illustrate lines of elaboration of a radial strobiloid type, with increasing size of the leaf. The division of Pteridophyta by Jeffrey, on anatomical characters, into small-leaved Lycopsida, and large-leaved Pteropsida is quoted; but it is concluded that the anatomical distinction of Jeffrey does not define phylogenetically distinct races, but is rather a register of such leaf-development as differentiated them from some common source. It is contended that the Ophioglossaceæ and Filices, which constitute Jeffrey's Pteropsida, are not necessarily akin on the ground of their large leaves, and consequent phyllosiphonic structure; but that they probably acquired the megaphyllous character along distinct lines. The opinion of Celakovsky is still held, "that the Lycopods are probably of living plants, the nearest prototypes of the Ophioglossaceæ." The more recent investigations of Jeffrey, and of Lang, have shown, however, that in the gametophyte of the Ophioglossaceæ, there is an assemblage of "Filicinean" characters, which differ from those of *Lycopodium* itself. But Celakovsky's comparison is *with the Lycopods, not with the genus Lycopodium*; so far as the facts go, increasing "Filicinean" characters of the gametophyte follow in rough proportion to the larger size of the leaf; thus from *Isoetes* we learn that a combination of cross characters is found in a megaphyllous Lycopod type. What we find in the Ophioglossaceæ is that in conjunction with their more pronounced megaphyllous form, still retaining, however, the Lycopodinous type of the sporophyte, they show more pronounced "Filicinean" characters of the gametophyte, and of the sexual organs. It is unfortunate that the facts relating to

the gametophyte of the Psilotaceæ and Sphenophylleæ are not available in this comparison.

It is not obvious what the meaning is of this parallelism between leaf-size and characters of the sexual organs; a further difficulty in its interpretation lies in the fact that for the Equiseta the parallelism does not hold; there "Filicinean" characters of the gametophyte accompany entirely non-Filicinean characters of the sporophyte, the latter showing nearer analogy to the Lycopods than to the Ferns. Such cross characters are difficult to harmonise with any phylogenetic theory; on account of them, the Equisetineæ are placed in an isolated position, and in the same way, though with less pressing grounds, a separate position should be accorded to those types which lie between the extremes of Lycopods and Ferns, in proportion as the characters are more or less pronounced.

On this basis the Isoetaceæ would probably best take their place as a sub-series of the Lycopodiales, Ligulatæ; the Psilotaceæ and Sphenophylleæ would constitute a series of Sphenophyllales, separate from, but related to, the Lycopodiales. The Ophioglossaceæ would form an independent series of Ophioglossales, more aloof than the latter from the Lycopodiales, but not included in the Filicales. The actual connection of these series by descent must remain open; it is quite possible that some or all of them may have originated along distinct lines from a general primitive group, which may be provisionally designated the Protopteridophyta; these were probably small-leaved strobiloid forms, with radial type of construction, and with the sporangia disposed on some simple plan. The grouping arrived at in these Memoirs may be tabulated as follows:—

PTERIDOPHYTA.

I. Lycopodiales.

(*a*) Eligulatæ.

Lycopodiaceæ.

(*b*) Ligulatæ.

Selaginellaceæ.
Lepidodendraceæ
Sigillariaceæ.
Isoetaceæ.

II. Sphenophyllales.

Psilotaceæ.
Sphenophyllaceæ.

III. Ophioglossales.

Ophioglossaceæ.

IV. Filicales.

(*a*) Simplices.

Marattiaceæ.
Osmundaceæ.
Schizaeaceæ.
Gleicheniaceæ.
Matonineæ.

(*b*) Gradatæ.

Loxsomaceæ.
Hymenophyllaceæ.
Cyatheaceæ.
Dicksonieæ.
Dennstaedtiinæ.
Hydropterideæ (?).

(*c*) Mixtæ.

Davallieæ.
Lindsayeæ.
Pterideæ, and other Polypodiaceæ.

V. Equisetales.

Equisetaceæ.
Calamarieæ.

"On the Negative Variation in the Nerves of Warm-blooded Animals." By N. H. Alcock, M.D. Communicated by A. D. Waller, M.D., F.R.S. Received January 17,—Read February 12, 1903.

(From the Physiological Laboratory, University of London, S.W.)

Introduction.

The negative variation in the nerves of warm-blooded animals has already been the subject of several researches.* While the nerves are still in connection with the tissues it has been the experience of most observers that there is no difficulty in examining the negative variation,

* Valentin, 'Pflüger's Archiv,' vol. 1, p. 423; Fredericq, 'Du Bois Archiv,' 1880, p. 70; Hermann, 'Physiologie,' vol. 2, p. 120; Gotch and Horsley, 'Phil. Trans.,' "Croonian Lecture," 1891, p. 267; Macdonald and W. Reid, 'J. Physiol.,' vol. 23, p. 100; Waller, 'Animal Electricity,' London, 1897; Boruttau, (*a*) 'Centralbl. f. Physiologie,' vol. 12, p. 317, 1898, (*b*) 'Pflüger's Archiv,' vol. 84, p. 309, 1901.

but with regard to isolated nerves contradictory statements have been made, and it was to ascertain if possible the reason of this discrepancy that the present research was undertaken.

Methods.

The following method was employed in all the experiments here quoted. The animal was killed by decapitation, and the body left undisturbed for 30—45 minutes. The nerves were then dissected out, placed in a 1·05 per cent. NaCl solution at about 30° C., and kept at this temperature for about half-an-hour or more. They were then allowed to cool to room temperature (17—19° C.), and it was found that, as a rule, the negative variation of nerves so treated was of the order of 1 millivolt (*vide infra*). This, of course, is not an absolute value of the true P.D. between active and inactive parts of nerve, but only a fraction of it, depending upon the amount of internal derivation in a nerve trunk by indifferent conducting tissue.

From about 2 to 6 hours *post-mortem* this value remains at a fairly constant amount, for instance, in Experiment B^d, the sciatic of the rabbit was used 5 h. 30 m. *post-mortem*, and gave a negative variation of 0·00083 volt; in Experiment A^k, 2 h. 40 m. *post-mortem* the value was 0·00076 volt, and these are typical instances. The table on p. 277 gives the result of twenty-two experiments, in which the values were taken for heat determination, which illustrates this.

Certain points may be here noted. The practice of placing nerves in salt solution for some time before use has been employed by Waller,* Gotch,† and Boycott,‡ in the case of the frog.§

The effect of changes in the composition of the salt solution is the subject of another research which I hope to publish at a future time; however, I may here state that small differences in the concentration of the solution—*e.g.*, ± 0·1 per cent. NaCl—make no apparent difference in the condition of the nerve, and the same is true in the main of *small* differences in the reaction, and of small differences of temperature.

Waller‖ has pointed out that the presence of lactose in the solution is of advantage, and taking a greater value of the negative variation for a sign of greater irritability, the same appears to be true in mammalian nerve for maltose and glucose, though I make the statement at present with some reserve.

For instance:—Experiments B^{tc} and B^{td}.

* Waller, 'Brain,' vol. 73, 1896, p. 43, *et seq.*
† Gotch, 'J. Physiol.,' vol. 28, p. 32.
‡ Boycott, *loc. cit.*
§ See also Gotch and Horsley, *loc. cit.* Macdonald and Reid, *loc. cit.*
‖ Waller, *loc. cit.* (Lectures), p. 73.

Young Rabbit.

Nerves kept for 3^h circa *post-mortem*.

Experiment No.	Temp. of nerve.	Volt. of neg. var. at excit. 100.	Volt. of neg. var. at excit. 30.	Notes.
B^{tc}	30° C.	0·00033	0·00023	1·05 p. c. NaCl + 0·5 p. c. maltose. R. sciatic.
B^{td}	30° C.	0·00023	0·00016	1·05 p. c. NaCl only. L. sciatic.

The two nerves of opposite sides are here compared, and the comparison is in favour of the R. sciatic which had been treated with maltose. No account is here possible of the precautions used to exclude fallacy, but many further experiments support the one quoted, and it is at least very probable that the maltose is the active variant.

The method employed for determining the negative variation and the action of anæsthetics was in all cases that of Waller. An additional larger box was used, with a false bottom of wood, on which the nerve chamber rested. Below this was a layer of water, so that the nerve was kept in an atmosphere nearly saturated with water vapour at whatever temperature was desired.

The standard "Berne" coil, worked with two Leclanché cells, was used to give the excitation, which always consisted of tetanising shocks for a period of 13 seconds, repeated once a minute. The number of units used was 500, when not otherwise stated. The usual precautions against current escape and electrotonus were carefully observed; this was found to be particularly necessary in the case of bird's nerve.

Half-grown rabbits were found very suitable animals to use, as the small amount of connective tissues permitted the nerves to be dissected out with a minimal amount of injury. Experiments were also made with cats, kittens, guinea-pigs, hedgehogs, pigeons, and frogs. The technique is considerably easier in the case of young animals; adults, however, answer well if care be taken. In the latter, and especially in the nerves of birds, it is advisable to work at rather higher temperatures than those given above.

Voltage and Strength of Excitation.

The voltage of the negative variation varies with the animal, the nerve employed, the temperature and condition of the nerve, and, within certain limits, with the strength of excitation.

The question of temperature will be considered later. Assuming for the present purpose that the conditions of experiment were equally favourable throughout, and taking the sciatic nerve as a standard, different animals gave the following values:—

	Millivolts.		
	Maximum observed.	Mean.	
Rabbit	1·1	0·69	Mean of 11 experiments.
Kitten	0·66	0·50	„ 5 „
Pigeon	1·05	0·42	„ 5 „
Guinea-pig	1·2	0·89	„ 2 „

All these are very much less than the frog, which gives 2 millivolts or more.

Different nerves in the same animal often show individual inequalities, but as a rule the larger nerves give a smaller negative variation than those of less diameter. The sciatic gives commonly the least, but is the most resistant to adverse influences. The median and ulnar nerves are more delicate, but give larger variation under favourable circumstances, *e.g.*, the median nerves of the pigeon gave a mean value of 0·54 millivolt (five experiments) as against 0·42 for the sciatic. The greatest value I have yet measured was in a branch of the anterior crural of the rabbit, which on the right side gave 2·5 millivolts and on the left 2·3.

Boruttau* found the vagus in the rabbit to give a larger negative variation than the sciatic, and obtained only very small responses from the nerves of hens, ducks, or pigeons.

The larger number of fibres not in contact with the longitudinal electrode would appear to act as a deriving circuit of less resistance in the larger nerves, and so less current passes through the galvanometer, and the greater amount of connective tissue in the sciatic would have the same effect. It is possible that there are other causes in addition to these; there is at present no evidence for or against such a possibility.

Similar reasons probably also explain why a stronger stimulus is necessary for the warm-blooded nerves than for the frog. The difference is, however, not very great. The minimal effective excitation I have so far observed is 6 units of the "Berne" coil, 500 units is commonly a maximum, 1000 nearly always so. The smaller electrical

* Boruttau (*a*), *loc. cit.*

resistance of the mammalian nerve between the exciting electrodes must be borne in mind in these comparisons.

Gotch* has recently stated that in determining the sub-maximal response of frog's nerve, the excitation of a smaller number of fibres is a far more potent cause than the varying response of each fibre, and it seems very probable that the higher threshold and wide range of excitation in mammalian nerves is due to the failure of the exciting current to reach the more distant fibres, protected as they are by intervening fibres and connective tissue, and not to any essential difference in the nerves.

The negative variation commonly persists without great alteration under ordinary conditions for at least 4—8 hours *post-mortem*. The longest time I have seen was in Experiment C^{df} (internal popliteal of the hedgehog, 28 hours *post-mortem*); the right and left median nerves the kitten in Experiments C^{eg} and C^{ef} gave a small and rapidly diminishing response 19 hours *post-mortem*.

The earlier observers (Valentin, Fredericq, Hermann) have stated that they have found the negative variation to persist for days, and to last longer than in frog's nerve. I am unable to confirm this; even in the hedgehog the nerves are much more short-lived than in the frog under similar conditions, and the phenomena referred to were probably of a different nature to those examined here, viz., electrotonic spread or ordinary diffusion.

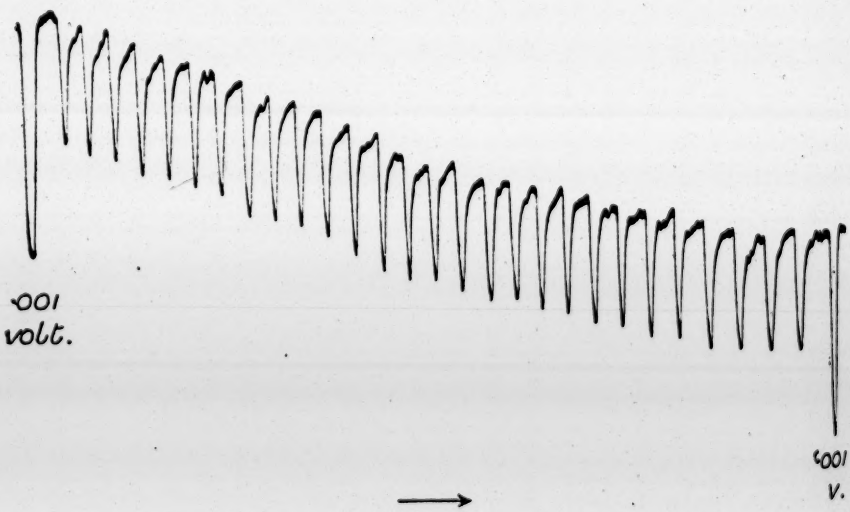


FIG. 1.—Rabbit. R. ext. popliteal. Normal series of negative variations. Exp. B^a. Reads from left to right.

* Gotch, 'J. Physiol.,' vol. 28, p. 40.

Action of Anæsthetics.

The following is the summary of the observations made on this subject:—

Chloroform.

Experiment and plate No.	Negat. var. before.	During $CHCl_3$.	After.	Notes.
204	$\frac{1}{1000}$ volt. 0·48	0·42 to 0 3 min.	0 to 0·42	Kitten. Sciatic. Temp. of nerve chamber, 32°. Complete abolition and recovery.
207	0·38	0·38 to 0 5 min.	0 to 0·22	Kitten. Sciatic. Temp. = 30° C. After $CHCl_3$ a positive variation appeared, changing again to a negative. Fair recovery.
A° 408	0·95	0·70 to 0·40 3 min.	0·21 to 0·13	Rabbit. L. sciatic. 6 h. *post-mortem*. Temp. = 20°. Gradual progressive diminution and no recovery. *FIG.* 3.
C^{cc} 424	0·46	0·46 to 0·15 4 min.	0·18 to 0·36	Kitten. L. median. Temp. = 32°. $CHCl_3$ dilute at first, stronger after first minute.
B^{l} 417	0·47	0·6 to 0·1 5 min.	0·21 to 0·40	Pigeon. R. median. 3 h. *post-mortem*. Temp. 37°. Recovery.

Ether.

201	0·40	During ether. 0·25 to 0 3 min.	0 to 0·28	Kitten. R. sciatic. Temp. 34°. 37 m. *post-mortem*. Imperfect recovery.
203	0·66	0·25 to 0 3 min.	0 to 0·46	Kitten. L. sciatic. 4 h. *post-mortem*. Temp. = 28°. Recovery.
B^{b} 413	0·54	0·22 to 0 3 min.	0 to 0·54	Rabbit. Ext. popliteal. 2 h. 30 m. *post-mortem*. Temp. = 30°. Recovery. *FIG.* 2.
B^{b} 416	0·83	— to 0 2 min.	0 to 0·51	Pigeon. 2 h. 30 m. *post-mortem*. Temp. = 39° C. L. sciatic. Recovery.

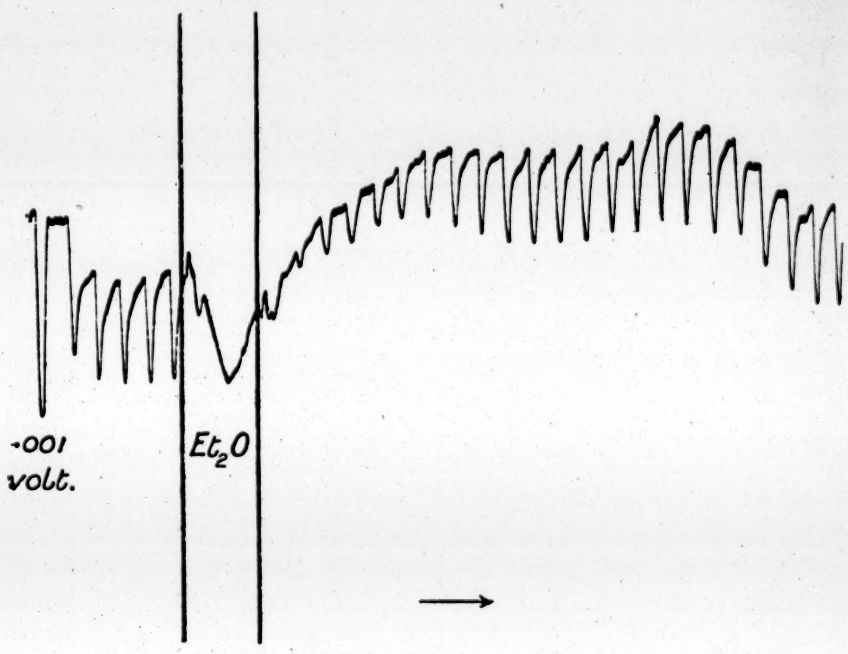


FIG. 2.—Rabbit. Same nerve as fig. 1. Ether vapour. Exp. B^b.

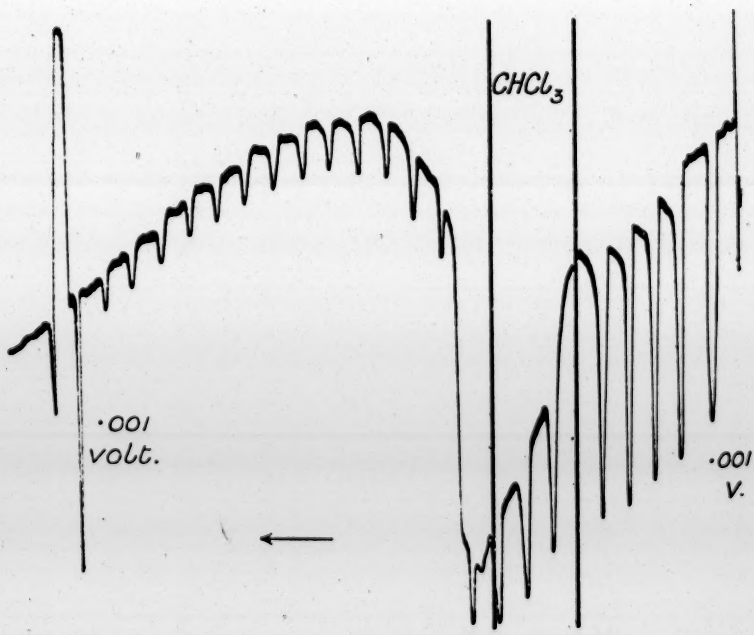


FIG. 3.—Rabbit. L. sciatic. Chloroform vapour. Reads from right to left. Exp. A^c.

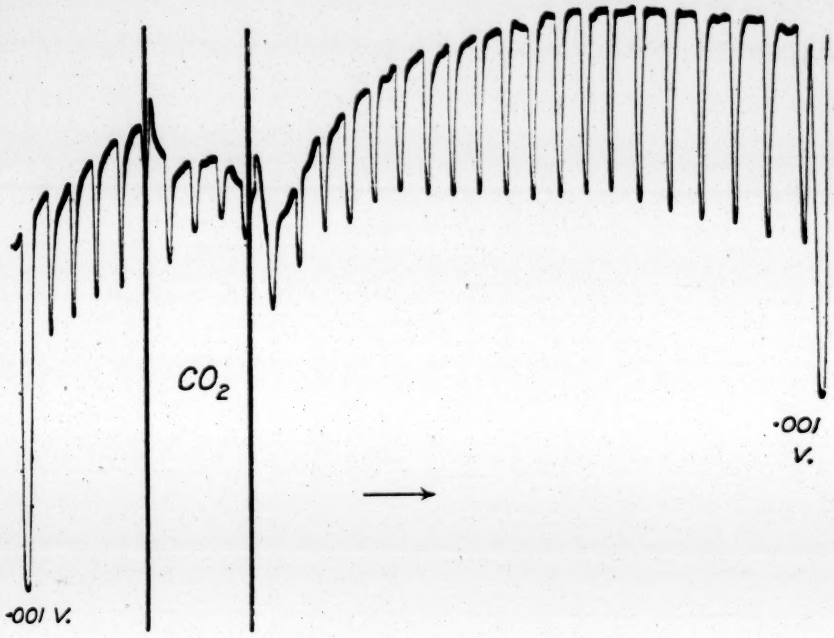


FIG. 4.—Rabbit. R. sciatic. CO_2. Exp. A[1].

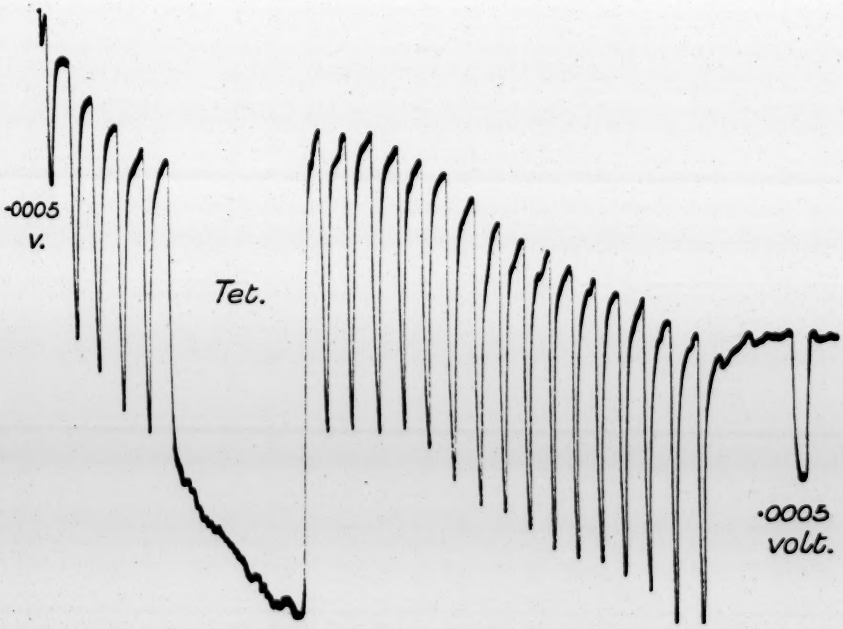


FIG. 5.—Rabbit. R. sciatic. Tetanisation for 5 m. Exp. A[1].

CO_2.

Experiment and plate No.	Neg. var. before in 0·001 volt.	Neg. var. during CO_2.	Neg. var. after.	Notes.
202	0·59	0·1 3 min.	0·3, 0·66, 0·53	Kitten. L. sciatic. 3 h. *post-mortem.* Temp. = 30°. Primary diminution and secondary augmentation.
A^l 406	0·39	0·29 to 0·15 4 min.	0·16 to 0·61	Rabbit. R. sciatic. 3 h. 45 m. *post-mortem.* Temp. 22° C. As 202. Marked secondary augmentation. *FIG.* 4.
A^j 405	0·70	0·56 to 0·3 5 min.	0·35 to 0·97	Rabbit. L. sciatic. 1 h. 10 m. *post-mortem.* As 406.
B^m 418	0·32	0·3, 0·42, 0·29 6 min.	0·64 to 0·33	Pigeon. L. sciatic. 3 h. *post-mortem.* Temp. = 37° C.

Tetanisation.

A^q 409	2·5	Tet. 5 min.	2·4	Rabbit. Branch of R. ant. crural. Temp. = 27°·5. 1 h. *post-mortem.*
A^r 410	2·3	Tet. 6 min.	1·8	Rabbit. Do., do. L. Temp. = 21°·5.
A^n 407	0·90	Tet. 5 min.	0·85 to 0·96	Rabbit. L. sciatic. Temp. = 20°.
A^t 411	0·86	Tet. 5 min.	0·92	Rabbit. R. sciatic. Temp. = 27°. *FIG.* 5. *Note.*—0·0005 volt compens. sent in after Tet. 2 h. 30 m. *post-mortem.*
C^{cc} 423	0·26	Tet 6 min.	0·26 to 0·19	Kitten. L. ulnar. Temp. = 36°·5 C. 3 h. *post-mortem.*

The values are taken from photographs. The action of these drugs on the nerves of warm-blooded animals is very similar to their effect on the nerves of the frog. Chloroform, ether, and carbon dioxide all produce diminution of the negative variation, followed by recovery in the case of the latter, with recovery or not in the case of $CHCl_3$, and the details of the process are clearly to be seen in figs.

The only difference that may be detected is that while in the frog the negative variation is increased by "little" $CHCl_3$ or Et_2O, and the abolition by "much" $CHCl_3$ and Et_2O is commonly followed by recovery to or beyond the normal; this increase has only been

observed in the bird (Experiment B^l) and not in the mammal. In the case of CO_2 this increase is seen in all the warm-blooded nerves; primary and secondary augmentation are shown in Exp. B^m from the pigeon, and the latter, in fig. 4, from the rabbit.

I do not here enter upon any discussion of the actual mechanism of this increase, it may be due to either of two causes—increase of E.M.F. or increased duration of electromotive change. The latter explanation has been suggested by Gotch as being the true one.

The effect of tetanisation (fig. 5) has not been marked. Three experiments proved negative, and two gave a slight increase, so that this question is still undecided.

It appears, therefore, certain that neither in the voltage of the negative variation, in the strength of excitation, or in the action of anæsthetics is there any marked difference between the warm-blooded and amphibian nerves, and that all the facts ascertained for the latter under these heads can be applied *en bloc* to the former.

Temperature of Extinction by Heat.

Three series of experiments on frogs, mammals, and birds were undertaken to ascertain the precise point at which the negative variation was abolished by heat.

Method.—The nerve chamber was kept at a constant temperature throughout, *e.g.*, 30° C. The nerve itself was placed on the electrodes, and when it had reached the temperature of the chamber, the value of 0·001 volt was determined on the galvanometer scale, and then the values of the first six negative variations. The nerve was then removed, placed in 1·05 per cent. NaCl solution (containing Ca salts, &c.) at the desired high temperature (*e.g.*, 49° C.), left for exactly 5 minutes, placed in cool (18° C.) salt solution for 7 minutes. A fresh transverse section was made, the nerve was replaced on the electrodes, and the value of 0·001 volt and the second set of six negative variations determined.

This method fulfils several *desiderata*.

(1.) It is possible to keep the beaker of hot saline solution at any given temperature with an error of less than ± 0·1° C.

A standardised thermometer was placed in the bath close to the nerve, and with 5 minutes immersion all parts of the nerve reach the temperature of the solution.

(2.) Any alteration in the resistance of the nerve is readily detected by means of the standard deflection with 0·001 volt, and as both readings are taken at the same temperature, this alteration must be a permanent one, and not the temporary alteration always seen when a nerve is heated or cooled.

(3.) Using a concentration of salt solution* that had been found to

* No carbohydrate was added to the solution in any of the heat experiments. The solution was neutral.

work equally well with all classes of nerve, and carefully preserving similar conditions of experiment, the results in the different series are strictly comparable not only with each other, but also with Halliburton's* researches on the heat-coagulation of the nerve proteids.

The excitation was a maximal one throughout.

Series I.

A. With Laboratory Frogs.
B. With freshly-caught vigorous Frogs.
Sciatic nerve. Excitation 30, except in A^e.

Experiment No.	Hours *post-mortem*.	Temp. of nerve chamber.	Temp. of hot bath.	Neg. var. initial = *a*. 0·001 v.	Neg. var. final = *b*.	Irritability quotient $= \frac{b}{a}$.	Notes.
	h. m.		°				
A^e	1 0	room	43·5	1·0	0	0	A.
A^f	2 7	,,	40·7	0·8	0	0	
A^{ha}	5 0	,,	40·0	1·2	0	0	
A^{hb}	5 30	,,	39·0	1·8	0·56	0·31	
A^g	2 30	,,	38·5	1·7	1·7	1·0	
A^z	6 0	17° C.	42·1	2·5	0	0	B.
A^w	2 40	,,	42·0	2·3	0	0	
A^y	3 30	,,	41·1	1·8	0·91	0·51	
A^v	2 0	15° C.	40·0	·7	3·0	1·7	

Series II.

Rabbit. Sciatic. Excitation 1000 for first three experiments, last four, 500.

Experiment No.	Hours *post-mortem*.	Temp. of nerve chamber.	Temp. of hot bath.	Neg. var. initial = *a*. 0·001 v.	Neg. var. final = *b*.	Irritability quotient $= \frac{b}{a}$.	Notes.
	h. m.	°	°				
B^{fa}	6 30	33·0	48·5	0·53	0·12?	0·22?	Doubtful return.
B^{ca}	3 30	29·0	48·0	0·57	0	0	
B^d	5 30	30·0	47·7	0·83	0·25	0·30	
A^u	4 0	20·5	46·0	1·1	0·45	0·41	
A^p	6 35	21·0	44·3	0·52	0·34	0·65	
A^m	5 0	19·0	42·3	0·48	0·52	1·1	
A^k	2 45	19·0	39·5	0·76	0·69	0·91	

* Halliburton. "Croonian Lecture," 1891, and below.

Series III.

Pigeon. Experiments B^n and B^s Sciatic, all the rest Median.

Experiment No.	Hours *post-mortem*.	Temp. of nerve chamber.	Temp. of hot bath.	Neg. var. initial $= a$. 0·001 v.	Neg. var. final $= b$.	Irritability quotient $= \frac{b}{a}$.	Notes.
	h. m.	°	°				
C^{fb}	3 15	28	53·6	0·30	0	0	Cold bath accidentally omitted. The nerve was much contracted longitudinally after heating.
C^{fc}	4 0	30	53·0	0·75	0	0	Nerve contracted. No "negative variation" after heating, but large positive current escape observed, not abolished by crushing.
B^n	4 15	38	52·5	0·25	0·12	0·48	Neg. var. rapidly diminishing.
B^k	4 30	38	52·0	0·56	0·17	0·30	
B^l	3 0	37	50·0	0·61	0·18	0·30	
B^s	2 0	30	45·3	0·30	1·05	3·5	

The experiments can be summarised thus :—

	Normal temp. of animal.	Temp. of incr. neg. var.	Temp of dim. neg. var.	Temp. of abolished neg. var.
Frog	—	39—40° C.	39—41°	40—42°
Rabbit.....	37—41°*	42·3°	44·3—47·7°	48—49°
Pigeon.....	40—42·5†	45·3°	50°	52—53°

It is seen that the effect of heat occurs in three stages. In the first, at a temperature of 1—2° above that of the animal, the negative variation is increased. In the second there is diminution, recovered from at the lower temperatures (4° over normal) if the nerve is cooled longer than the standard time, not recovered from at the higher (6—7° over normal), and finally the negative variation is permanently abolished, 8° over normal in the rabbit, 10° in the pigeon.

While the mammalian and avian nerves show quite small individual

* Pembrey (Schäfer's 'Text-book,' vol. 1, p. 790). The higher limit for the rabbit is from unpublished observations of Dr. Pembrey, which he has very kindly furnished me for this paper.

† Corin and Van Beneden, 'Arch. de Biol.,' Gand., 1887, vol. 7, p. 265.

differences in different animals as regards their reaction to heat, the frog's nerve varies a little according to the condition of the animal, and so the observations have been arranged in two divisions. Here one also notices that the "injury range" is very much smaller than in the warm-blooded nerve, 2° at most separating a temperature that has no ill effect for one that finally kills the nerve, as against 5—6° in the mammal and bird.

Observations.

A summary of the previous work on the effect of temperature on nerves is to be found in Howell's* paper, and in that of Boycott (*loc. cit.*).

Howell, from his own researches, gives 41—44° as the temperature at which conductivity is abolished in frog's nerve, the other authors give 45—50°. The difference appears to be due to the methods employed. Hitherto, there has been some difficulty in ensuring that all parts of the nerve shall have the same temperature, and this temperature has in most cases been ascertained indirectly, further, the time during which the temperature is kept up and the conditions of moisture, &c., greatly influence the results.†

Another explanation is possible. The majority of observers‡ have examined the conductivity of nerve as opposed to the excitability, and if the two processes are supposed to be distinct, it might be said that the excitability was extinguished before the conductivity. In view of the considerations stated above, and also of the relation to the coagulation point of the proteids, this hypothesis does not seem to be well founded.

The relationship of the extinction point given above and the coagulation point of the proteids in the body of the animal is a very close one. In the frog, the first coagulation of extracted muscle proteid occurs at 40° C,§|| the first step in heat rigor of the muscle itself at 38—40°,¶ the electrotonic currents are abolished at 40°,** and the extinction point of the nerves as determined above, 40—42° C.

In the rabbit the proteid coagulation occurs at 47°,§|| the muscle

* Howell, Budgett and Leonard, "J. Physiol.," vol. 16, p. 298.

† Some earlier experiments I have made under different conditions lend support to these remarks.

‡ Except Edwards' 'J. Hopkins Lab. Studies,' vol. 4, 1887, p. 18 (45—48° —55°?); and Moriggia, 'Moleschott's Untersuchungen,' vol. 14, p. 382 (46—47°).

§ Halliburton, *loc. cit.*; also Halliburton and Mott, 'Archives of Neurology,' vol. 2.

|| Von Fürth, 'Arch. f. Exper. Path. u. Pharmak.' Leipzig, 1895, vol. 36, p. 231, and *ibid.*, vol. 37, 1896, p. 389.

¶ Vincent and Lewis, 'J. Physiol.,' vol. 36, p. 445; see also Brodie and Richardson, 'J. Physiol.,' vol. 21, 1897, p. 353, and 'Phil. Trans.,' B, vol. 191, 1899, p. 127; and also Vernon, 'J. Physiol.,' vol. 24, p. 239.

** Waller, 'Roy. Soc. Proc.,' vol. 60, p. 384.

rigor at 45—50°C,* and the nerves die at 48—49°. The proteids of the cat's brain coagulate at 47° C.† No data for the bird are available,‡ the nerves die at 53°.

In table form.

	Frog.	Mammal.	Bird.
Muscle proteid (Halliburton and von Fürth)	40°	47°	—
Muscle rigor (Vincent and Lewis).....	38—40°	45—48°	—
Nerve proteid (Halliburton).....	—	47°	—
Nerve electrotonic currents (Waller)	40°	—	—
Nerve (present experiments).....	40—42°	48—49°	53°

It is reasonable to conclude from these figures that the extinction of the irritability of the nerve is due to the coagulation of the proteids which enter into its composition, and I venture to forecast, that when the proteids of the frog's nervous system are examined one will be found to coagulate at 40°, and that the two proteids coagulating at 40° and 47° are absent from the nerves of the bird. It is possible, therefore, to make a nearer approach to the analysis of actually living nerve substance than has been practicable hitherto.

Temperature of Extinction by Cold.

Method.—Alongside the nerve in the nerve-chamber, was placed a junction (A) of konstantan and iron wire, and the nerve was arranged so as to touch this. The junctions konstantan copper (B) and iron-copper (C) were placed in glass tubes and immersed in water at room-temperature, the two copper terminals led to a key-board, with connections to a sensitive Kelvin-type galvanometer of low resistance (16 ohms), and a compensating circuit arranged as shown. The wire rheochord marked 1 ohm was of the ordinary du Bois-Reymond type, and with the voltage and added resistance as marked 1° difference between the junction A, and B C, was represented by about thirty-five scale divisions. The compensating current was furnished from an accumulator of large capacity. It was found after careful tests that no perceptible alteration (within 0·05°) of the

* Vincent and Lewis, 'J. Physiol.,' vol. 36, p. 445; see also Brodie and Richardson, 'J. Physiol.,' vol. 21, 1897, p. 333, and 'Phil. Trans.,' B, vol. 191, 1899, p. 127; and also Vernon, 'J. Physiol.,' vol. 24, p. 239.

† Halliburton, *loc. cit.*; also Halliburton and Mott, 'Archives of Neurology," vol. 2.

‡ Demant, 'Zeitschr. f. Physiol. Chemie,' vol. 3, p. 241, and Kühne and Chittenden, 'Zeitschr. f. Biol.,' N. F., vol. 7, p. 358, 1889, have made some observations on this point, but I have been unable to consult the papers.

temperature of the fixed junction took place, if it did, a correction could be readily applied to the figures obtained. To guard against current escape from one circuit to another all the wires leading to the nerve chamber were placed within rubber tubing, and the konstantan-iron junction (A) was coated with rubber "tyre-repairing" solution, which on drying left a thin and even coat of rubber on the surface, insulating it from any nerve currents and from any possible mutual action from or to the nerve. It was found by experiment that no such action occurred.

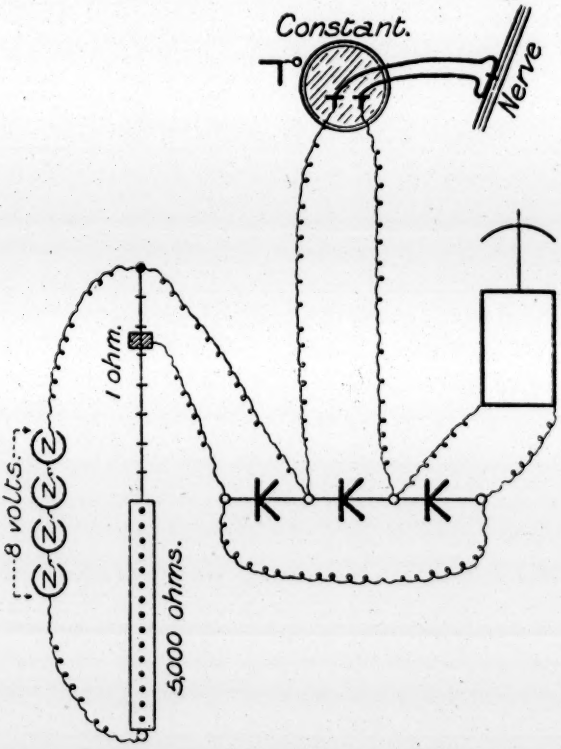


FIG. 6.

To graduate the instrument the junction A was placed in melting ice, the thermo-current compensated till the galvanometer spot stood at zero, and the compensator scale read off. This was repeated with hot water when it was desired to use a temperature higher than the constant of B C. This graduation was performed as a matter of precaution at the commencement of each day's experiments. The whole apparatus proved simple and convenient to use, and of an accuracy much in excess of what proved necessary in these experiments. And as the junction A actually touches the nerve, there can be no doubt that the actual temperature of the latter is observed. The readings are given to 0·1°C. The nerve chamber was cooled by being placed in a tin box, outside which was a layer of ice and salt,

Experiments.

Abstract.

Animal.	Experiment No.	Initial neg. var. 0·001 volt.	Nerve.	Extinction point.	Limits.	Notes.
Frog	B^p	2·0 q.p.	Sciatic	−3°·5 C.	−3°·5 C.	
Hedgehog*	C^{da}	0·12	Ext. popliteal	+6°·4	+ 6·4 to −1° 4	C^{de} was a 2nd experiment with the same nerve as C^{de}.
„	C^{de}	0·40	Int. popliteal	+2°·6		
„	C^{df}	0·44	„	−1°·3		
„	C^{de}	0·32	„	−1°·4		
Rabbit.....	B^{ub}	0·89	Int. popliteal	+7°·4	+7°·4 to +3°·8	
„	C^{de}	0·54	Ext. popliteal	+7°·1		
„	C^{db}	0·60	Int. popliteal	+3°·8		
Pigeon.....	C^{ea}	0·45	Median	+8°·2	+8°·2 to +6°·9	
„	C^{eb}	0·75	Median	+6°·9		

* Dr. Pembrey very kindly furnished me with these animals in a state of hibernation.

using the same apparatus that Waller* employed in his researches of the effect of temperature on the electrotonic current of frog's nerve.

The negative variation was observed in the way before mentioned. The value of the galvanometer deflection was ascertained by taking the scale value of 1 millivolt at intervals. It was found, as is well known, that the resistance of the nerve and electrodes gradually increased as the temperature was lowered, and this causes a small error in the strength of excitation, though this was annulled as far as possible by an added resistance of 100,000 ohms in the exciting circuit.

Comments.

The limits determined are for the temporary abolition of the negative variation, not for its permanent abolition. There is a gradual rise of the extinction point through the four classes of amphibians, hibernating mammals, mammals and birds. The limit varies a little in each experiment in a manner that is not accounted for by either the apparent condition of the nerve or by its anatomical character. That some variation was to be expected was clear from the researches of Howell,† who found that the vaso-constrictor fibres in the cat's sciatic were paralysed by cold (+4° C. *q.p.*), while the vaso-dilatator fibres were paralysed 1° lower, and even greater differences were observed between the cardiac and respiratory fibres in the vagus.

Taking the experiments as they stand, it is evident that those upon which most reliance can be placed, are where the nerve has reached

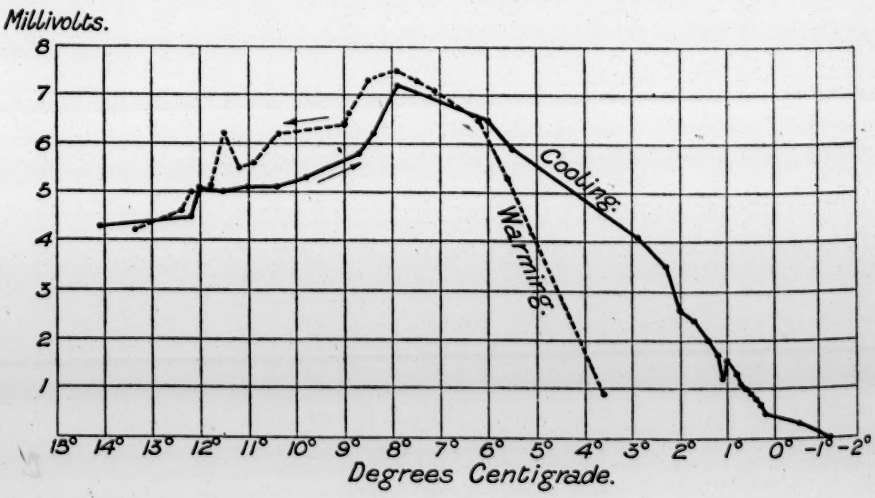


FIG. 7.—Exp. C^{df}. Value of the negative variation in nerve of hedgehog at different temperatures.

* Waller, 'Proc. Physiol. Soc.' in 'J. Physiol.,' vol. 20.
† Howell, *loc. cit.*

the *lowest* point before extinction of the negative variation took place, and both the frog and hedgehog agree in giving a measurable response below 0°. In experiment B on the frog the negative variation reached a maximum at +3°·8 C. In the experiment C^{df} on the hedgehog an exactly similar maximum was observed at +7°·9 C., and plotting out the "cooling" and "warming" curves the latter gave also a maximum at the same point (fig. 7).

No such maximum was certainly observed in the mammal or pigeon. There were traces of a maximum at 25° C. in the former (in experiment B^{ub} and an earlier experiment on the kitten not recorded above), but the experiments C^{dg}, C^{dh}, C^{ed}, C^{eh} showed no sign of this. Several explanations are possible, but it seems preferable to await the result of further experiments before insisting too strongly on any of them. One, however, seems well established, that the negative variation follows the temperature with a certain "lag." This is seen to a small extent in the nerve of the hedgehog (fig. 7), in the rabbit and bird it is larger in amount, and tends to obscure curves taken in this way.

I have not yet determined the *permanent* extinction point, recovery took place in experiment B^{p} in the frog after a temperature of −3°·5 C. had been reached, and in Experiment B^{ub} on the rabbit (−2°·5), experiments are in progress in this direction.

The range of temperature through which the nerve can function is obtained by combining the figures here observed with those of the former series, and it is found that this range is the same for all the nerves examined, 45°·5 for the frog, 45°·2 for the rabbit, and 46°·1 for the pigeon, one step higher in the temperature scale in each case (Fig. 8).

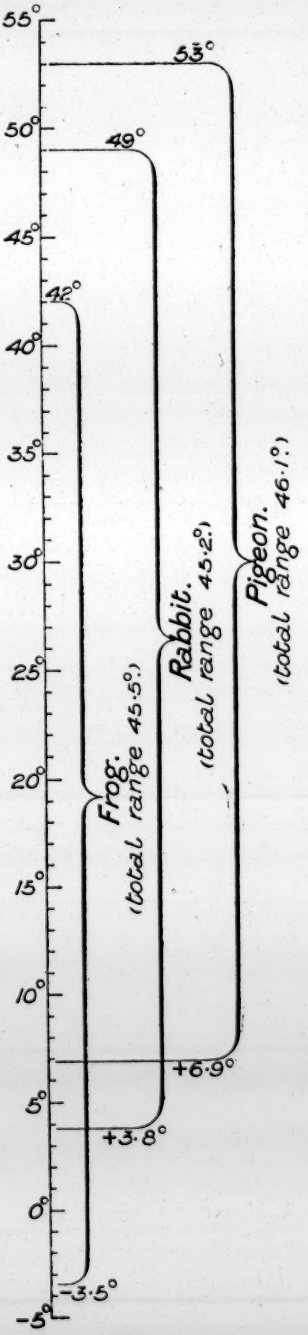


FIG. 8.

Conclusion.

(1.) It is possible to examine isolated mammalian and avian nerves under the same conditions as frog's nerves.

(2.) There is no essential difference between the nerves of frogs, mammals, and birds as regards their negative variation, excitability, and reaction to anæsthetics.

(3.) There is a marked difference in the extinction point for heat. The negative variation in frog's nerve is abolished at 40—42° C., in rabbit's nerve at 48—49°, in pigeon's nerve at 53°.

(4.) This extinction point corresponds closely with the first coagulation point of the body proteids, where these are known, and thus coagulation is probably the cause of the permanent loss of irritability of the nerve.

(5.) The point at which the nerves are paralysed by cold is −3°·5 in the frog, −1°·4 in the hedgehog, +3°·8 in the rabbit, and +6°·9 in the pigeon.

It gives me great pleasure to acknowledge my indebtedness to Dr. A. D. Waller for his great kindness and assistance in everything connected with this paper.

"On the Decline of the Injury Current in Mammalian Nerve, and its Modification by Changes of Temperature. Preliminary Note."

By S. C. M. Sowton and J. S. Macdonald. (From the Thompson-Yates Laboratory of Physiology, University College, Liverpool.) Communicated by Professor C. S. Sherrington, F.R.S. Received December 10, 1902,—Read February 12, 1903.

The sciatic nerve of a freshly-killed frog led off from transverse section and longitudinal surface to a galvanometer, gives a current of injury which, as Engelmann* and others have pointed out, is greatest immediately after the section has been made. If tested at frequent intervals, it is found that from the outset the E.M.F. rapidly diminishes. A continuous record of the decline of the current may be obtained, by photographing the movement of the galvanometer spot, using for the purpose the method devised by Dr. Waller, and fully described in his papers.† The nerve with its electrodes being inclosed in a moist chamber, such an observation may be prolonged almost indefinitely. The curve is convex to its abscissa, the decline being rapid at first and gradually diminishing in speed.

If the injury current of fresh mammalian nerve be examined in a similar way, the records show in many cases a marked difference of curve, the decline being often very gradual; some records even

* Engelmann, 'Pflüger's Archiv,' vol. 15, pp. 116—148.

† S. C. M. Sowton, 'International Congress,' Cambridge, 1898.

exhibiting an actual rise, so that the highest value of the injury current is only attained when the nerve has lain for some time upon the electrodes. Several observers* have noted the initial rise, and explanations have been offered which accord with the generally held "alteration" theory of the nerve current. In face, however, of facts recently brought forward in a series of papers by one of us,† it is difficult to accept the prevailing theory as satisfactory. The attempt was therefore made to study this particular phase of the injury current from the point of view set forth in the papers alluded to. That view, which may be called the "concentration cell" theory of the injury current, is based upon the hypothesis of the core-model structure of nerve,‡ and lays stress on certain pre-existing peculiarities of constitution. These may briefly be described as (*a*) a separation of the solutions of electrolytes of the nerve into internal and external solutions by a membrane which permits only imperfect diffusion to take place between them; (*b*) a difference in the quantitative distribution of electrolytes in the solutions, the internal one being of small volume, but of great concentration and high specific conductivity. Such a difference between the solutions must give rise on rupture of the membrane, as by section or other injury, to diffusion processes, and consequently to differences of potential.

If such electrical differences as are found in the phenomenon of the injury current arise from the source indicated, they should be capable of modification in just the same manner that a process of diffusion can be modified. The value of a diffusion process depends primarily upon the concentration ratio of the two solutions in contact, and may be increased by diluting the weaker solution. In the experiments already reported upon,§ the extreme case of this dilution was exemplified by immersing the nerve for a short time in water; the result was an increase of the injury current due to the enhanced value of the diffusion process.

In relation to the experiments recorded here, it may be said that the temperature of the solutions, or the fact of any difference of temperature existing between them, is hardly less important than the concentration. In frog's nerve, the solutions are already approximately at the temperature of the laboratory. But with mammalian nerve the case is very different. Removed immediately after death, such a nerve has a temperature presumably not far removed from that of the

* Waller, "Croonian Lecture," 'Phil. Trans.,' London, 1896.

† J. S. Macdonald, "The Source of the Demarcation Current considered as a Concentration Cell," 'Proc. Roy. Soc.,' vol. 67, p. 315, &c., 1900, &c.; "The Injury Current of Nerve. The Key to its Physical Structure," 'The Thompson Yates Laboratories' Reports,' vol. 4, part 2, 1902, pp. 213—350.

‡ Grünhagen, 'Konigsberger Med. Jahrb.,' vol. 4, p. 199; Strong, 'Journal of Physiology,' vol. 25, p. 427; Boruttau, 'Pflüger's Archiv,' vol. 63, p. 154, &c., &c.

§ J. S. Macdonald, 'The Thompson Yates Laboratories' Report,' *loc. cit.*

mammalian body. Cooling must, therefore, continue after the nerve has reached the electrodes; and this cooling is not a perfectly simple matter, for the fatty sheath of the nerve is not only, probably, a bad conductor of electricity, but also undoubtedly an indifferent conductor of heat, we should expect, therefore, the cooling process to be differential, the temperature of the external solution falling sooner than that of the internal solution or axis cylinder; and since cooling is in a manner equivalent to dilution, the concentration ratio of the two solutions should be at first increased, and with it the value of the E.M.F.

The theory then affords an explanation of the increase observed when a nerve just out of the body is examined at a lower temperature. It remained to further exemplify this fact by attempting to reproduce the phenomenon at will. An experiment was so arranged that the proper temperature of the nerve was artificially maintained, and then at a given moment allowed to fall gradually. The resulting curve shows that at or near body temperature the injury current declined rapidly; in this particular case it fell in half-an-hour 44 per cent. of its original value. In the second portion of the record *during the period of cooling*, the rapid fall was arrested, there was a period of hesitation, and then a slow rise of the current. On the temperature being again raised, the current resumed its rapid decline. In such an experiment the alteration of condition obtained by fall of temperature is comparable to that produced by dilution of the external solution, and the results are in complete agreement.

It is not surprising that to obtain such an initial rise *it is necessary to restrain the fall of temperature within certain limits*, outside of which the effect is masked by a more powerful factor working in the direction of diminution. Where, for example, a nerve fresh from the body is transferred to a temperature of 0° C., there is no initial increase; for here the *difference* of temperature between nerve and surroundings being great, the rate of cooling will be rapid, and internal solution as well as external will be quickly affected, and to cool the internal solution is equivalent to diminishing its concentration and hence to a lowering in value of the diffusion process.

Extreme cooling should, indeed, annul the production of the injury current, by arresting the processes of diffusion upon which its manifestation depends. Yet, in a cooled nerve, the *source* of the phenomena, the concentration ratio, is preserved, and a raising of the temperature ought again to develop the injury current. This expectation is fulfilled experimentally. At a temperature near 0° C. the current declined rapidly, but it regained its value to a great extent as the temperature was increased. With a nerve maintained at body temperature the injury current fell rapidly. A high temperature favours diffusion (*i.e.*, the equalising of the solutions), and the phenomenon

is reduced by exhausting the value of the source. A low temperature reduces the value of the injury current by checking the process upon which it depends. Both extremes, then, reduce the value of the current, but by quite different means. This being so, there will be a mean temperature around which these two effects are balanced; at which the value of the source decreases less rapidly than at the higher temperature, and the value of the diffusion process is greater than at the lower temperature, at which, therefore, the injury E.M.F. is best maintained. This consideration led to the systematic study of the injury current at different steady temperatures, and the sought-for point of best maintained E.M.F. was found to lie between 14° and 19° C.

In the experiments hitherto considered, we have dealt with *current*, no allowance being made for changes of resistance brought about by altered temperature. The error, in some cases, was of no great moment, in others, those, namely, where a lowering of temperature gave an increased injury current; a correction for altered resistance would but have accentuated the point it was sought to establish. But there are instances in which the error might be serious—and it seemed, therefore, desirable throughout the inquiry to supplement the photographic records by a series of observations, in which measurements of potential by compensation should be taken at frequent intervals. Such measurements have been undertaken by us, and completely confirm the statements made above.

"On the Formation of Definite Figures by the Deposition of Dust." By W. J. Russell, Ph.D., F.R.S. Received January 29,—Read February 19, 1903.

(Abstract.)

The author shows that when a plate of glass or other material is slightly warmed and allowed to cool for 6 or 7 minutes in a dust-laden atmosphere, a clear and definite figure is formed on the plate. The figure is determined by the form of the plate on which it is deposited. If a square plate is used then a simple cross is formed, a ray of deposit proceeds from each corner of the plate to the centre. If the plate be triangular, a ray again proceeds from each corner; and with an octangular plate an eight-rayed star is formed. In every case the number and position of the angles of the plate determine the form of the figure. The dust generally used was that produced by burning magnesium ribbon, but any fine dust acts in the same way and produces the same figures.

With regard to the plate on which the figure is deposited, its composition is not of importance except as a back ground for the dust. A glass plate for many reasons is best, but the figures form with equal



certainty and sharpness on one of copper, or mercury, or ebonite, or India-rubber, or card-board, &c. In order to heat the plate it may be passed several times over the flame of a lamp, warming it as uniformly as possible, and, if it be a glass plate, until the moisture condensed on the under side has disappeared; or the plate may be heated by laying it on a copper plate heated to about 120°C. for 30 minutes, or it may simply be warmed in an air or water bath. The plate is best supported on three pieces of wire about $1\frac{1}{2}$ inches long, and a receiver filled with the dust, inverted over it and allowed to remain there for 6 or 7 minutes.

In order to obtain symmetrical figures the plate on which they are deposited must be perfectly horizontal, and as they are very sensitive to heat, there must be no unequal heating either of the plate or the surrounding atmosphere while the deposition is taking place.

As long as the plate and the surrounding atmosphere are nearly of the same temperature only very imperfect figures form, but as the temperature rises a more and more nearly perfect figure appears. If the plate be above 17°, indications of pictures are produced when the plate is at a slightly lower temperature than the surrounding atmosphere, but when the difference is 6° or more, these indications cease altogether. Very good pictures are produced by having the plate at 12° or more degrees above the dust-laden air, and even when the plate is 100° or 120° above the air, distinct but thin pictures are produced. The effect of a slight heat below the plate, while the deposit is taking place, is shown to thicken the figure, and distort it in a curious manner and is illustrated by photographs. Also the effect of radiant heat on these figures is shown by the action of a Bunsen burner at distances of 12 and 26 inches, and of other sources of heat at considerable distances from the plate. Some singular and complicated effects are

produced by placing a source of heat above the plate instead of below it. A large number of experiments are also recorded and illustrated showing the effect which different bodies in the immediate neighbourhood of the plate have on the figures which are formed. Taking only one case, that of a pin. When it is placed in contact and at right angles to the plate a definite deposit is produced, and this varies as the pin is moved further and further away, and as it is placed either on a level with plate, or above or below it. Even when the pin is 6 mm. below the level of the plate, and 2 mm. away from it, a distinct effect is produced. Again, these dust currents may be influenced in a remarkable way by suspending glasses of different sizes, and at different heights above the plate on which the figures are depositing, and photographs of the figures produced are given. The effects produced by obstructions of different sizes laid on different parts of the plate are also shown.

It was also found that a current of dust drawn through a tube will form a characteristic figure on a plate, which need not be warmed, as it passes over it.

If the magnesia dust be allowed to settle on a surface of water, about the temperature of 17° C. or, on water containing a very small amount of alcohol or glycerine, the deposit which forms on the surface breaks up, by the powder sinking, into a figure of cellular form.

Magnesia dust, which was generally used, undergoes some strange changes. When first deposited it is removed by the slightest touch, but if the plate be kept for a week or fortnight it may then be softly brushed over without damage to the figure. Another change which this powder undergoes is shown by collecting it immediately it forms, and examining it under a microscope, when it will be found to consist of irregular shaped and separate particles, but if the collection of the dust be made a few minutes after its formation, it is then seen that the particles are strung together, forming small and irregular fibres. In the various figures that have been produced the magnesia seems to have assumed this form.

"Mathematical Contributions to the Theory of Evolution.—On Homotyposis in Homologous but Differentiated Organs." By KARL PEARSON, F.R.S., University College, London. Received January 20,—Read February 19, 1903.

(1.) In the paper on "Homotyposis in the Vegetable Kingdom,"* I defined homotypes as "undifferentiated like organs." In the course of that paper, I endeavoured to indicate that I was not unconscious of the influence of age, local environment, and position upon organism in modifying homotypic correlation. The object of my memoir, however, was to obtain some general appreciation of the average intensity of individuality in living forms, and to see if it approached the average value of fraternal heredity in plant or animal life. For this purpose I selected such material as was readily available, indicating the series where I thought differentiation of a sensible amount was present owing to the age, the situation, or the environment factors.

From the standpoint of theory, however, we are not compelled to adopt a mere indication of this kind. As soon as we can correlate between: (*a*) age and the quantitative character of the homologous organs, (*b*) situation on the organism and this same character, or (*c*) local environment and the character, we can allow for the differentiation of homologous parts, or reduce them to pure homotypes. In other words, homotyposis can be deduced from differentiated homologous parts, if we correct for the differentiation due to (*a*), (*b*) or (*c*). The test for the existence of such differentiation is simply the presence or absence of the corresponding correlation.

We have accordingly the following problems to find solutions for:—

(i.) To find the correction to be made to the apparent homotypic correlation, when the pairs of homologous parts are differentiated from each other by their periods of growth.

(ii.) To find the correction to be made to the apparent homotypic correlation, when each pair of homotypes is differentiated by a common period of growth from other pairs of homotypes.

(iii.) To find the correction to be made to the apparent homotypic correlation when the pairs of homologous parts are differentiated from each other by situation on the organism.

(iv.) To find the correction to be made to the apparent homotypic correlation when each pair of homotypes is differentiated by the environment of its organism from other pairs of homotypes.

It will be seen that in problems (ii) and (iv) we are dealing with true homotypes, but that the homotypic factor requires modifying for the influence of age or environment on the organism. In (i) and (iii)

* 'Phil. Trans.,' A, vol. 197, pp. 285—379.

we are not dealing with homotypes at all, but with homologous parts, and we wish to reduce them to homotypes by correcting for differences between them due to growth or to situation on the organism.

I propose at present to deal only with problems (i) to (iii), not because (iv) does not admit of theoretical treatment, but because we have not thus far obtained data to illustrate satisfactorily the correlation between character and the immediate environment of the individual organism. Experimental determinations of homotyposis in plants, when the individuals are subjected to a graduated environmental scale, *e.g.*, in depth of soil or quantity of moisture allowed would be fairly easy to carry out, and most interesting in result. I hope it may be possible to arrange experiments of this kind for the coming summer. We can then illustrate the fourth proposition from actual observation, and the publication of its theoretical solution will be of greater value.

(2.) *To find the correction to be made to the apparent homotypic correlation when the pair of homologous parts are differentiated from each other by their periods of growth.*

Let x and y denote the characters in the two homologous parts quantitatively determined, and t_1, t_2 their respective periods of growth. Then we have four variable quantities x, y, t_1, t_2, no one of which fixes absolutely any other, for individuals will have different characters even with the same period of growth. The proposition accordingly reduces to this: What is the correlation R between x and y for constant values of the variables, *i.e.*, selected values of, t_1 and t_2?

This problem is answered in formulæ (lviii), (lix) and (lx) of my memoir: "On the Influence of Natural Selection on the Variability and Correlation of Organs."*

Let us write in those formulæ t_1 for the subscript 1, t_2 for 2, x for 3, and y for 4; we have at once

$$\Sigma_x^2 = \sigma_x^2 \frac{1 - r_{t_1t_2}^2 - r_{xt_1}^2 - r_{xt_2}^2 + 2r_{t_1t_2}\, r_{xt_1}\, r_{xt_2}}{1 - r_{t_1t_2}^2} \quad \text{(i)},$$

$$\Sigma_y^2 = \sigma_y^2 \frac{1 - r_{t_1t_2}^2 - r_{yt_2}^2 - r_{yt_1}^2 + 2r_{t_1t_2}\, r_{yt_2}\, r_{yt_1}}{1 - r_{t_1t_2}^2} \quad \text{(ii)},$$

$$\Sigma_x\Sigma_y \mathrm{R} = \sigma_x\sigma_y \frac{r_{xy}(1 - r_{t_1t_2}^2) - r_{xt_1}\, r_{yt_1} - r_{xt_2}\, r_{yt_2} + r_{t_1t_2}(r_{xt_1}\, r_{yt_2} + r_{yt_1}\, r_{xt_2})}{1 - r_{t_1t_2}^2} \quad \text{(iii)}.$$

Now if we deal with direct and not cross-homotyposis, *i.e.*, with the *correlation* of the *same* character in two homologous parts, we can put these results more simply. We in this case render our correlation tables symmetrical by entering each one of a pair of homologues first as an x and then as a y. We may then write

* 'Phil. Trans.,' A, vol. 200, p 30.

$$r_{xt_1} = r_{yt_2} = r, \qquad r_{xt_2} = r_{yt_1} = r',$$
$$r_{xy} = \rho, \qquad r_{t_1t_2} = \mathbf{r}, \qquad \sigma = \sigma_x = \sigma_y,$$

and we find

$$\Sigma_x^2 = \Sigma_y^2 = \sigma^2 \frac{1 - \mathbf{r}^2 - r^2 - r'^2 + 2\mathbf{r}rr'}{1 - \mathbf{r}^2}$$

$$\mathrm{R} = \rho \frac{1 - \mathbf{r}^2}{1 - \mathbf{r}^2 - r^2 - r'^2 + 2\mathbf{r}rr'} - \frac{2rr' - \mathbf{r}(r^2 + r'^2)}{1 - \mathbf{r}^2 - r^2 - r'^2 + 2\mathbf{r}rr'} \quad \ldots\ldots \text{(iv)}.$$

This is the full solution of the first problem.

We see that in order to solve it, it is necessary:

(i.) To find the correlation ρ of the homologous pairs as if they were simple homotypes.

(ii.) To find the correlation $\mathbf{r}$ between the growth periods of each pair of homotypes.

(iii.) To find the correlation r between the character and the period of growth.

(iv.) To find the correlation r' between the character of one homotype and the period of growth of its fellow.

Now these correlations can be found at once by the usual statistical processes, if the data are forthcoming.

(3.) I propose to illustrate this on material, which, although not homotypic, is so analogous that it brings out all the important features.

We will determine the correlation between the head-length of brothers, such length being measured on school boys of all ages, from 4 to 19.* It will be clear that we have here all the difficulties of the homotypic problem—resemblance due to common origin obscured by differences in the period of growth of each individual.

Table I gives the correlation of pairs of brothers without regard to their differences of age.

Table II gives the correlation between age and length of head in the same individual.

Tables IIIA and IIIB gives the correlation between the age of one brother, and the length of head of the second.

Table IV gives the correlation between the ages of pairs of brothers.

These tables have been prepared by taking off from the brother-brother data papers of my school measurement records all the available pairs of cases falling into each series. Thus in some cases the ages of both brothers were given, but not the head measurement of one or other; in other cases the head measurements of both, but the age of one or other would fail, or again the age of one and the head measurement of the other might be all the information available. Thus the total number of cases and the frequency distribution varies slightly from one table to a second.

* The measurements form part of the material obtained with the assistance of a grant from the Royal Society Government Grant Committee.

A few remarks must be made on these tables.

Table I gives the following values of the constants:—

Mean length of head of elder brother	=	186·7508 in mm.
„ „ younger „	=	183·8296 „
Standard deviation of elder brother	=	7·5027 „
„ „ younger „	=	7·3536 „

The correlation is, then, found to be 0·601,751,* and the regression, younger on elder brother, 0·5897. These give the intensity of heredity, uncorrected, for the growth factor.

Now, the most noteworthy part of this result is, as we shall see later, that *taking brothers at different ages tends to exaggerate the apparent intensity of heredity.* If we were to take pairs of boys at ages from 4 to 19, each pair having no hereditary relationship, but being, on the average, within a year or so of the same age, we should find a spurious correlation due to the mixture of material, each pair having approximately-like head-lengths because the members of it were, approximately, of like age. On the other hand, if the boys were blood relations of very different ages, their apparent relationship would be weakened, because we should be correlating the same organ at different stages of its growth. We have thus two factors: one tending to exaggerate, and the other to weaken the apparent strength of hereditary resemblance. It is of great interest to note that the former factor in the present case is the more effective.

In Table II we have what I term a growth table, *i.e.*, a correlation table between period of growth and the quantitative measure of a character. The constants of this table are as follows:—

Mean age of boy	=	13·0394 years.
Standard deviation of age.....	=	2·8207 „
Mean head-length	=	185·4516 mm.
Standard deviation of head-length	=	7·4991 „
Correlation of age and head-length	=	0·453,496

The regression coefficient for head-length on age = 1·205676, and we have the probable head-length H_P for observed age A given by

$$H_P = 169·7303 + 1·2057\,A \quad \ldots\ldots\ldots\ldots\ldots\ldots (\epsilon.)$$

Thus, on the average, boys' heads grow in length 1·2 mm. a year.

My results are based on 1637 cases entirely taken off the brother-brother data papers. Dr. Alice Lee at an earlier stage also worked out a growth table. We had not then so many brother-brother data papers filled in. She used in addition all the brother measurements on the brother-sister papers, and so reached 1856 boys, of which, I

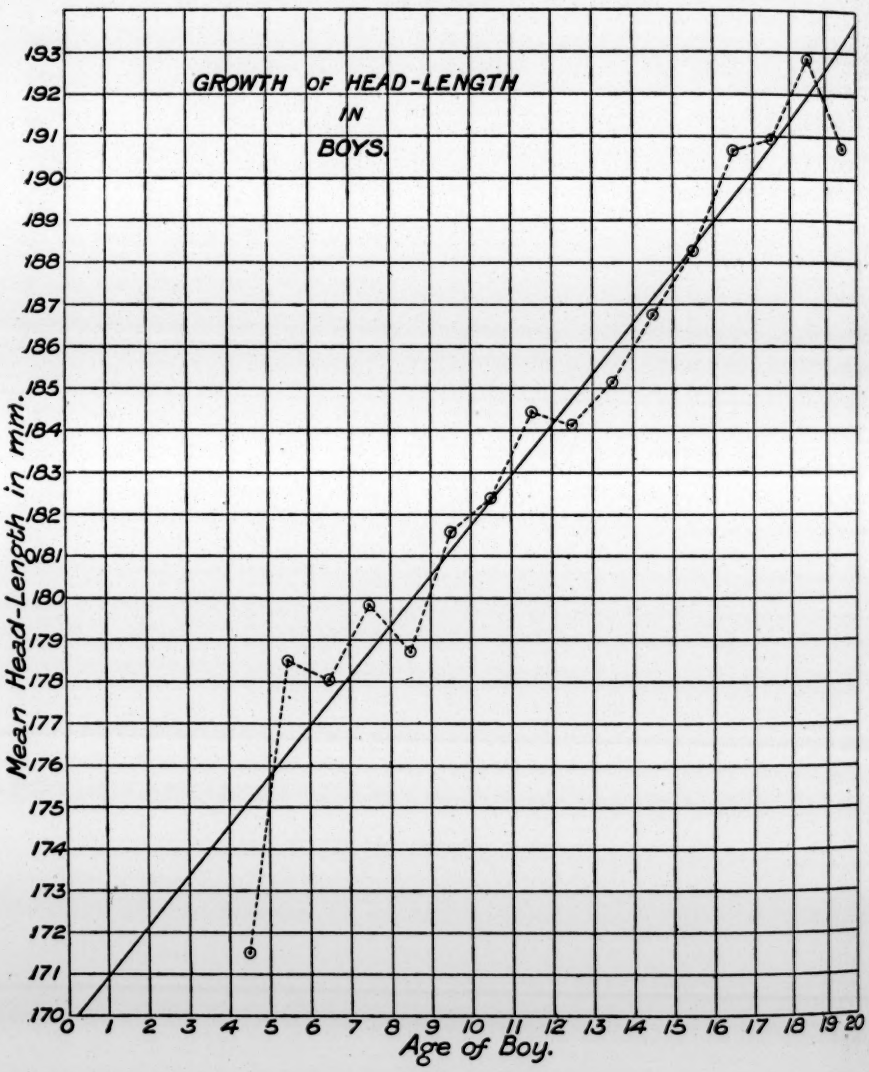
* Six figures have been kept in the correlation coefficients, as we require to calculate the regression coefficients from the differences of products and powers.

think, we may safely assert that 400 at least are not included in my series. She found: Mean age,* 12·7177; mean head-length, 184·8182; and slope of regression line, 1·2040, giving the formula

$$H_P = 169{\cdot}5061 + 1{\cdot}2040\,A,$$

a result in substantial agreement with mine.

DIAGRAM 1.



In Diagram 1 the formula (ϵ) is represented with the observed mean values at each year of life. The results for the 4th, 5th, and 19th years of life ought not to be considered, for they are based on only 2, 10, and 12 observations respectively. It will clearly hardly be possible to express the growth curve better than by a straight

* The mean age is less, because brother-sisters are obtained chiefly from primary, not secondary, schools.

line, until the range of data is very largely extended. The regression is sensibly linear.

Table IIIA and Table IIIB give the following results :—

Mean age of elder brother ..	= 14·1249	Mean age of younger brother	= 11·7149
S.D. of elder brother's age..	= 2·5124	S.D. of younger brother's age	= 2·7221
Mean head-length, younger brother	= 183·8578	Mean head-length, elder brother	= 186·6515
S.D. of head-length, younger brother	= 7·2806	S.D. of head-length, elder brother	= 7·5005
Correlation of age of elder and head-length of younger ..	= 0·396,598	Correlation of age of younger and head-length of elder .	= 0·379,326

We see accordingly that within the limits of the probable error, the correlation between younger brother's head-length and elder brother's age is the same as that between elder brother's head-length and younger brother's age. This result might, to some extent, have been anticipated, but actual proof of this type of cross-relation is of value. In Table IV we have the correlation between ages of brothers giving the constants :—

Mean age of elder brother	= 14·1508
Mean age of younger brother	= 11·7487
S.D. of elder brother's age	= 2·5080
S.D. of younger brother's age	= 2·7220
Correlation of brothers' ages	= 0·884,186

The first four results are in good agreement with those of Tables IIIA and IIIB. The last result shows how nearly there is an approximation to a constant difference in age between brothers in schools. Very closely we have—

Probable age of younger brother = 0·96 × (age of elder brother) − 1·83.

When the elder brother is 6, his younger brother is probably 2·1 years younger than he is; when the elder brother is 12, the younger brother is probably 2·3 years younger, and when he is 18, 2·6 years younger. The explanation of this is that when the elder brother is very young only his near or second brother will, as a rule, be at the same school, but in the secondary schools, which he reaches at a much later age, it is possible for a much younger brother to be at the same school.

Now let us substitute the correlation values, found in equations (i) to (iii), of page 290. We have

$$r_{xy} = 0\cdot601,654, \qquad r_{t_1t_2} = 0\cdot884,186$$
$$r_{xt_1} = r_{yt_2} = 0\cdot453,496,$$
$$r_{xt_2} = 0\cdot379,326, \qquad r_{yt_1} = 0\cdot396,598.$$

Whence we find

$$\Sigma_x/\sigma_x = 0{\cdot}890{,}051, \qquad \Sigma_y/\sigma_y = 0{\cdot}891{,}209,$$

and

$$\mathrm{R} = 0{\cdot}5446.$$

This is a very reasonable value of fraternal correlation, agreeing quite well with results obtained for horse, man and dog. It is worth noting that

$$r_{xt_1} \times r_{t_1t_2} = r_{yt_2} \times r_{t_1t_2} = 0{\cdot}4010,$$

and, therefore, either equals r_{xt_2} or r_{yt_1} fairly closely; in fact, within the probable error of their difference.

Hence, it would appear highly probable that the cross-relation between one brother's head length and a second brother's age is solely due to the correlation of the ages between the two brothers.

If such a result as

$$r_{xt_1} \times r_{t_1t_2} = r_{xt_2} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (\eta)$$

should be verified on the reduction of further data, it will enable us to much simplify our formulæ.

Thus we easily find for this case

$$\Sigma_x = \sigma_x \sqrt{1 - r_{xt_1}{}^2}, \qquad \Sigma_y = \sigma_y \sqrt{1 - r_{yt_2}{}^2}$$

and

$$\mathrm{R} = \frac{r_{xy} - r_{xt_1}{}^2 r_{t_1t_2}}{1 - r_{xt_1}{}^2}.$$

Or, we require to find only the uncorrected correlation (ρ) the growth correlation (r), and the correlation between periods of growth (r). The correction to be made to the apparent correlation is then the subtraction from it of

$$\frac{r^2(\mathrm{r} - \rho)}{1 - r^2}.$$

I hope shortly to ascertain whether relations like η above hold also for other head-measurements on growing children.

Table I.—Collateral Heredity Head Length in Brothers, uncorrected for Age.

*Head-length of Elder Brother in mm.**

Head-length of Younger Brother in mm.	162—3.	164—5.	166—7.	168—9.	170—1.	172—3.	174—5.	176—7.	178—9.	180—1.	182—3.	184—5.	186—7.	188—9.	190—1.	192—3.	194—5.	196—7.	198—9.	200—1.	202—3.	204—5.	206—7.	208—9.	Totals.
162—3	—	—	2	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	3
164—5	—	—	—	—	—	—	—	—	1	—	—	—	1	—	—	—	—	—	—	—	—	—	—	—	2
166—7	—	—	—	—	1	1	0·5	1·5	—	—	—	—	—	—	—	—	0·25	0·25	—	—	—	—	—	—	4·5
168—9	—	—	1	0·5	1·5	1	1	1	2	1	1	1	1	1	—	—	0·25	0·25	—	—	—	—	—	—	13·5
170—1	1	—	—	4	3	3	—	3	4	2	3	3·5	3	3	—	—	1	—	—	—	—	—	—	—	33·5
172—3	—	—	—	1·75	4·25	2	2	3	1	2	2	2·5	1	1	—	1	—	—	—	—	—	1	—	—	24·5
174—5	—	—	1	0·75	3·25	1	—	5	5	9	3·5	8	2	5	—	1	1	—	—	—	—	—	—	—	45·5
176—7	—	—	—	—	1	1	6	3	8	5·5	10·5	10	4	3	2	—	1	—	1·5	—	—	—	—	—	56·5
178—9	—	—	—	—	2	3	2·5	6·5	11	8	5	11·5	8	5·5	4·5	2	3	1	1·5	—	—	—	—	—	75
180—1	—	—	—	5	1·5	—	2	4	11	17	13·5	14·5	16	14	6·5	8·5	6	2·5	—	—	1	1	—	—	119·5
182—3	—	—	—	—	2	2	2·5	4·5	8	9	13	14·5	8·5	12	11	10	2	1·75	1·25	1	—	—	—	—	103
184—5	—	—	—	—	—	—	—	2	1	11·5	12	17	15·5	17·75	9·75	8·25	7·25	4·25	3·25	2	2	—	—	—	113·5
186—7	—	—	—	—	—	1	2	2	2	6	12·5	7	8	15·25	17·75	11·75	8·25	5	1	—	—	—	—	—	99·5
188—9	—	—	—	1	—	1	1·5	1·5	2	4	5	7	6	9·5	12	6·5	7·5	4·5	1	1	1	—	—	—	72
190—1	—	—	—	—	—	—	—	—	—	2	1	3·5	8·5	10·5	9·5	7·5	10·5	10	3	2	—	2	—	—	70
192—3	—	—	—	—	—	—	—	—	2	1	2	2	4	7	10·5	7·5	8	6	7	1·5	—	1	—	1	60·5
194—5	—	—	—	—	—	—	—	—	—	1	1	1	1	5·5	4	3	2·5	5	5	3·5	1	1	—	—	34·5
196—7	—	—	—	—	—	—	—	—	—	—	—	1	1	1	3·5	2	5	0·5	3·5	—	—	1	—	—	18·5
198—9	—	—	—	—	—	—	—	—	—	—	—	—	2	—	—	2	2	4	2	4	2	1	—	—	19
200—1	—	—	—	—	—	—	—	—	—	—	—	—	1	—	1	—	—	1	1	2	1	1	1	—	9
202—3	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	—	—	1	2	—	—	4
204—5	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1·5	0·5	—	—	1	—	—	—	—	—	3
206—7	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	—	—	1
208—9	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	—	—	—	—	—	—	—	1
Totals	1	0	4	9·5	19·5	16	20	37	58	79	85	104	92·5	112	93·5	71·5	65·5	47	32	17	9	11	1	1	986

* The group 162—3 contains all boys with head-lengths greater than 161·5 and less than 163·5 and so on. When a head-length fell exactly on such a value as 183·5 it was halved between the columns 182—3 and 183—4.

Table II.—Correlation of Age and Length of Head in Boys.

*Year of Age of Boy.**

Length of Head in mm.†	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	14.	15.	16.	17.	18.	19.	Totals.
162—3	—	—	—	—	1	1	—	—	—	—	—	1	—	—	—	—	3
164—5	—	—	—	—	—	—	—	1	1	—	—	—	—	—	—	—	2
166—7	—	—	1	0·5	2	1	1	—	1	—	—	—	—	—	—	—	6·5
168—9	—	—	2	1·5	2	4	2	2	1	0·5	—	1	—	—	—	—	16
170—1	1	1	2	2·5	3	4	4	5·5	6	5·5	3	1	—	1	—	—	39·5
172—3	1	—	—	1·5	7	4·5	3	2·5	11	5	1	2	—	—	—	—	38·5
174—5	—	1	0·5	4	6	5·5	9	4	8	2·5	3·5	3	1	1	—	—	49
176—7	—	—	0·5	10	4	10	12	6	13	12	4·5	4	1	—	—	—	77
178—9	—	5	2	2	10·5	10	10	18	14	11·5	9	5	5	1	—	—	103
180—1	—	—	2	5	7·5	22	23	17	17	27	18	9·5	8	5	—	—	161
182—3	—	3	1	4	9	6	15	22	26·5	25	17	16	6	2	3	2	157·5
184—5	—	—	1	4	3·5	10·5	19	18	31	36	25	23	13·5	6	1	1	192·5
186—7	—	—	1	5	6·5	13	24	16·5	24·5	24	17·5	13·5	3·5	8	1	—	158
188—9	—	—	1	1	3	5·5	16	15	23	20	22·5	17	19·5	9	9	1	162·5
190—1	—	—	1	—	—	7	6	14·5	22	18·5	19·5	19·5	16	9	4	0·5	137·5
192—3	—	—	—	2	—	5	2	10	12	18·5	17·5	18	10·5	6·5	6	4·5	112·5
194—5	—	—	—	1	—	1	—	7	11	8·5	3	18·5	15·5	6·5	4	1	77
196—7	—	—	—	—	1	—	2	3	3	7·5	11	7	9	4	6	1	54·5
198—9	—	—	—	—	—	—	—	3	3	3	7	8	4·5	12	1	1	42·5
200—1	—	—	—	—	—	1	—	2	—	—	2	5	5·5	3	1	—	19·5
202—3	—	—	—	—	—	—	—	—	1	1	1	1	3·5	1	1	—	9·5
204—5	—	—	—	—	—	—	—	—	—	1	—	2	6	2	1	—	12
206—7	—	—	—	—	—	—	—	1	—	—	—	—	1	—	1	—	3
208—9	—	—	—	—	—	1	—	—	—	—	—	1	—	1	—	—	3
Totals..	2	10	15	44	66	112	148	168	229	227	182	176	129	78	39	12	1637

* All boys from the nth to the $(n+1)$th anniversary of their birthday would be placed in the nth column, or the mean age of such boys $= n + 0{\cdot}5$ years.

† See Note to Table I.

Table IIIA.—Correlation of Age of Elder and Head-length of Younger Brother.

*Year of Age of Elder Brother.**

Head-length of Younger Brother in mm.†	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	14.	15.	16.	17.	18.	19.	Totals.
162—3	—	—	—	—	—	—	—	1	1	—	—	—	—	—	—	—	2
164—5	—	—	—	—	—	—	—	—	1	—	1	—	—	—	—	—	2
166—7	—	—	—	—	1	—	1	1	1	0·5	—	—	—	—	—	—	4·5
168—9	—	—	—	—	—	2	1	6	2	2·5	1	—	—	—	—	—	14·5
170—1	—	1	1	—	2	1·5	2	5·5	7·5	6	1·5	3	1	—	—	—	32
172—3	—	—	—	—	2	0·5	4·5	3·5	6	2·5	2·5	2	2	—	—	1	26·5
174—5	—	—	—	—	1·5	3	4·5	3	10	7·5	6·5	2	3	1	—	—	42
176—7	—	—	—	—	1·5	2	8	8	6·5	10	4·5	8	2	2·5	—	—	53
178—9	—	—	2	—	1	2	10	9	15·5	11	8	5	8	2·5	—	—	74
180—1	—	—	—	—	1	5	14	12	20·5	16	16	14	9·5	7·5	5	2	122·5
182—3	—	1	—	—	2	4	5	7	14	19	8	17	12	8·5	1	3	101·5
184—5	—	—	—	—	—	—	5·5	5	16·5	15·5	18	21	17·5	9·5	5	2	115·5
186—7	—	—	—	—	—	2	4·5	9	20	19	10	15	8	3·5	7	—	98
188—9	—	—	—	—	1	3	2	7	8	16	7·5	5·5	10	10·5	5	1	76·5
190—1	—	—	—	—	—	—	—	4	6·5	10·5	12·5	9·5	7	11·5	5	1	67·5
192—3	—	—	—	—	—	—	3	1	3	10	8	9	13	7·5	5	3	62·5
194—5	—	—	—	—	—	—	1	—	4	3	5	3	5	3·5	5	4	33·5
196—7	—	—	—	—	—	—	—	—	3	2	2	6	4	2	—	—	19
198—9	—	—	—	—	—	—	—	—	—	2	2	3	6	3	1	1	18
200—1	—	—	—	—	—	—	—	1	—	1	—	—	1	2	2	—	7
202—3	—	—	—	—	—	—	—	—	—	—	—	—	1	1	1	—	3
204—5	—	—	—	—	—	—	—	—	—	—	—	1	1	1	—	1	4
206—7	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	—	1
208—9	—	—	—	—	—	—	1	—	—	—	—	—	—	—	—	—	1
Totals..	—	2	3	—	13	25	67	83	146	154	115	124	111	77	42	19	981

* See Note to Table II.

† See Note to Table I.

Table IIIB.—Correlation of Head Length of Elder and Age of Younger Brother.

*Year of Age of Younger Brother.**

Head-length of Elder Brother in mm.†	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	14.	15.	16.	17.	18.	19.	Totals.
162—3	—	—	—	—	—	—	—	—	—	1	—	—	—	—	—	—	1
164—5	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	0
166—7	—	1	—	—	—	2	—	—	—	1	—	—	—	—	—	—	4
168—9	—	—	1	2	1	2·5	0·5	—	1·5	—	—	—	—	—	—	—	8·5
170—1	—	1	—	1	4	2·5	2·5	2	3·5	1	—	1	—	—	—	—	18·5
172—3	—	1	—	2	2	3	2	2	2	2	—	—	—	—	—	—	16
174—5	—	—	2	3	2	3·5	2	—	3·5	2	1	1	—	—	—	—	20
176—7	—	1	2	5	4	8·5	3	5	4·5	1	2	1	—	—	—	—	37
178—9	1	2	2	4	8	10	6	8	6	4	3	3	—	—	—	—	57
180—1	—	2	2	4	5	9·5	6·5	16	9	8	6	3	—	—	—	—	71
182—3	—	1	2	5	9	14·5	14	11	8	14·5	5	2	1	2	—	—	89
184—5	—	1	1	10	12	12·5	14	12	20·5	6·5	10	4·5	4	—	—	—	108
186—7	—	—	3	9	8	14·5	18	8·5	12	10	4	3·5	2	—	—	—	92·5
188—9	2	1	2	4	6	12·5	11·5	18	12·5	16·5	7	11	8	—	—	—	112
190—1	—	2	2	4	3	9·5	11	11	10	12·5	12·5	9	1	1·5	—	—	89
192—3	—	1	1	4	1	3	7	10·5	7	11	6·5	11·5	5·5	1·5	1	—	71·5
194—5	—	—	—	3·5	2	4	8	7	6·5	9	12	9·5	4·5	2	—	—	68
196—7	—	—	—	0·5	1	2	1	5·5	5·5	9·5	7	10	2	1	—	—	45
198—9	—	—	—	1	—	2	1	3·5	2	2·5	9	7	3	1	—	—	32
200—1	—	—	—	—	—	1	1	0·5	1	2	2	6·5	—	—	—	—	14
202—3	—	—	—	—	—	1	—	0·5	1	1	2	1·5	1	—	—	—	8
204—5	—	—	—	—	—	—	—	1	3	1	6	—	1	—	—	—	12
206—7	—	—	—	—	—	—	—	—	—	—	2	—	—	—	—	—	2
208—9	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	1
Totals	3	14	20	62	68	118	109	122	119	116	97	86	33	9	1	—	977

* See Note to Table II.

† See Note to Table I.

Table IV.—Correlation of Ages of Pairs of Brothers.

*Years of Age of Elder Brother.**

Year of Age of Younger Brother.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	14.	15.	16.	17.	18.	19.	Totals.
4	—	1	—	—	1	—	1	1	—	—	—	—	—	—	—	—	4
5	—	1	2	—	3	—	3	2	2	—	—	—	—	—	—	—	13
6	—	—	1	—	4	4	5	5	1	—	—	—	—	—	—	—	20
7	—	—	—	—	4	10	10	14	8	8	2	2	—	—	—	—	58
8	—	—	—	—	—	10	26	13	16	6	1	—	—	—	—	—	72
9	—	—	—	—	—	1	17	35	32	16	7	2	1	1	—	—	112
10	—	—	—	—	—	—	1	17	55	26	8	3	2	—	—	—	112
11	—	—	—	—	—	—	—	—	28	58	25	11	7	—	—	—	129
12	—	—	—	—	—	—	—	—	3	41	39	16	11	6	—	1	117
13	—	—	—	—	—	—	—	—	—	—	30	60	14	6	2	2	114
14	—	—	—	—	—	—	—	—	—	—	5	27	43	18	7	5	105
15	—	—	—	—	—	—	—	—	—	—	—	2	32	32	12	5	83
16	—	—	—	—	—	—	—	—	—	—	—	—	1	17	14	2	34
17	—	—	—	—	—	—	—	—	—	—	—	—	—	1	5	5	11
18	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	1
19	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	0
Totals	—	2	3	—	12	25	63	87	145	155	117	123	111	81	40	21	985

* See Note to Table II.

This table contains only fifteen pairs of twins, one pair of brothers having been born within ten months of each other. Any age, as 9, means all falling within the ninth year, *i.e.*, from the ninth birthday to the day before the tenth birthday, so that two brothers, not twins, might appear of the same age.

(4.) *To find the correction to be made to the apparent homotypic correlation, when each pair of homotypes is differentiated by a common period of growth from other pairs of homotypes.*

The solution of this problem may be deduced at once from equation (iii) of the preceding problem by simply putting $t_1 = t_2$. In this case $r = r'$, $\mathbf{r} = 1$, and we find

$$R = \frac{\rho - r^2}{1 - r^2} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \text{(v)}.$$

This equation was given by me in a note in *Biometrika*, vol. 1, p. 404, and its use illustrated on Dr. Simpson's data for *Paramecium caudatum*.

(5.) *To find the correction to be made to the apparent homotypic correlation when the pair of homologous parts are differentiated from each other by situation on the organism.*

We have only to put in formula (iii) on p. 290, $t_1 = p_1$ and $t_2 = p_2$, the positional co-ordinates of the first and second homologous parts, to make that formula available for position instead of age differentiation. If we denote by c_1 and c_2 the characters of the parts in the positions p_1 and p_2 respectively, our solution takes the form below, where we have confined our attention to the same character,

$$R = \frac{\rho(1 - r_{p_1p_2}{}^2)}{1 - r_{p_1p_2}{}^2 - r_{p_1c_1}{}^2 - r_{p_1c_2}{}^2 + 2r_{p_1p_2}r_{p_1c_1}r_{p_1c_2}} - \frac{2r_{p_1c_1}r_{p_1c_2} - r_{p_1p_2}(r_{p_1c_1}{}^2 + r_{p_1c_2}{}^2)}{1 - r_{p_1p_2}{}^2 - r_{p_1c_1}{}^2 - r_{p_1c_2}{}^2 + 2r_{p_1p_2}r_{p_1c_1}r_{p_2c_2}} \ldots \text{(vi)}.$$

This follows since $r_{p_1c_1} = r_{p_2c_2}$, and $r_{p_1c_2} = r_{p_2c_1}$.

We have again, therefore, to find four correlation coefficients. But this formula simplifies immensely if we observe the following conditions:

(*a.*) Take the same number of homotypes or homologous parts from the same positions in each organism.

(*b.*) Enter each one of these homotypes or homologous parts with each other on the same organism, so as to obtain a symmetrical table, *i.e.*, c_1 is first entered with c_2 and then c_2 with c_1,

These conditions are or can be usually satisfied in any homotyposis investigation.

(6.) Further, the positions will, as a rule, be arranged in series and may be numbered 1, 2, 3, 4,..m, if m homotypes or homologous parts be taken from each individual organism. The position scale is, of course, perfectly arbitrary, and has nothing to do, for example, with the actual distances between positions on the organism. We can make it a uniform numerical scale, which for convenience we can take to be the same serial order as that of positions on the organism.

Let $\bar{p}$ = mean position, σ_p = standard deviation of positions on the arbitrary position scale. Let there be n organisms, and suppose that S_m denotes a summation of all m homologous parts on an organism, and S a summation for all n organisms. Then, if $\sigma_p = \sigma_{p_1} = \sigma_{p_2}$,

$$nm(m-1)r_{p_1p_2}\sigma_p^2 = S_n\{S_m(p_1-\bar{p})(p_2-\bar{p})\}$$
$$= S_n\{S_m(p_1-\bar{p}) \times S_m(p_2-\bar{p}) - S_m(p_1-\bar{p})^2\}$$

But $S_m(p_1-\bar{p}) = 0$, hence, since $S_m(p_1-\bar{p})^2 = m\sigma_p^2$,

$$nm(m-1)r_{p_1p_2}\sigma_p^2 = -nm\sigma_p^2,$$

or
$$r_{p_1p_2} = -\frac{1}{m-1} \quad \text{..... (vii).}$$

Further

$$nm(m-1)r_{p_1c_2}\sigma_p\sigma_c = S_n\{S_m(p_1-\bar{p})(c_2-\bar{c})\}$$
$$= S_n\{S_m(c_2-\bar{c}) \times S_m(p_1-\bar{p}) - S_m(c_1-\bar{c})(p_1-\bar{p})\}$$
$$= -S_n\{S_m(c_1-\bar{c})(p_1-\bar{p})\}.$$

But
$$S_n\{S_m(c_1-\bar{c})(p_1-\bar{p})\} = nm\,\sigma_c\sigma_p r_{c_1p_1}.$$

Hence

$$r_{p_1c_2} = -\frac{r_{c_1p_1}}{m-1} = r_{c_1p_1} \times r_{p_1p_2} \quad \text{..... (viii),}$$

a relation precisely similar to that discovered in the case of growth periods for brother's head-lengths from the actual numbers on p. 294.

Substituting we find

$$1 - r_{p_1p_2}^2 - r_{p_1c_1}^2 - r_{p_1c_2}^2 + 2r_{p_1p_2}r_{p_1c_1}r_{p_1c_2} = (1-r_{p_1p_2}^2)(1-r_{p_1c_1}^2)$$
$$2r_{p_1c_1}r_{p_1c_2} - r_{p_1p_2}(r_{p_1c_1}^2 + r_{p_1c_2}) = r_{p_1p_2}r_{p_1c_1}^2(1-r_{p_1p_2}^2)$$

Then substituting in (vi) and using (vii) we determine the simple formula for homotyposis corrected for positional differentation

$$R = \frac{\rho}{1-r_{pc}^2} + \frac{1}{m-1}\frac{r_{pc}^2}{1-r_{pc}^2} \quad \text{..... (ix).}$$

where r_{pc} stands for the correlation of character and position on the organism.

An exactly similar formula might be found for the correction for the age or growth factor, if the m homologous parts dealt with had the same distribution of ages or growths in each organism.

(7.) Now the equation just found has the serious disadvantage that it is based on the linearity* of the regression relation between position

* The reader should note that this condition does not involve any assumption of normal frequency, or the Gaussian law. The latter applies only to a very special case of linear regression.

and character. But while organic and homotypic correlations give for a surprising variety of cases sensibly linear regression relations, the relation between position and mean character is far more rarely linear. We obtain, as a rule, remarkably smooth curves. We, therefore, require some modification of equation (ix).

Still supposing the regression of character and position linear, we should have, if σ' be the mean standard deviation of an array of organs in the same position,

$$\sigma'^2 = \sigma^2(1 - r_{pc}^2).$$

But if σ_M be the standard deviation of the means of the arrays, we have from first principles

$$\sigma^2 = \sigma_M^2 + \sigma'^2$$

Hence

$$\sigma_M = \sigma \times r_{pc}.$$

We can now write equation (ix) in the form

$$R = \rho \frac{\sigma^2}{\sigma^2 - \sigma_M^2} + \frac{\sigma_M^2}{(m-1)(\sigma^2 - \sigma_M^2)} \quad \ldots\ldots\ldots\ldots \text{(x)}.$$

This is quite free from r_{pc}, and, what is more, although we have deduced it from (ix) and the relation $\sigma'^2 = \sigma^2(1 - r_{pc}^2)$ peculiar to linear regression, it is now free of any limitation as to the nature of the relation between position and mean character. Thus (x) is a far more important formula than (ix), and should always be used, until we have shown that the relation between position and mean character is sensibly linear. If anything, it involves less arithmetic than (ix).

We can show this *ab initio* as follows:—Let the individuality of the organism in any homologous part be measured by its excess above (respectively defect below) the mean value of the character for the homologous part in that position.* Then, if c' = element of character due to individuality, and $\bar{c}_p$ be the mean character in any position for the n individuals dealt with,

$$c_1' = c_1 - \bar{c}_p, \quad S_n(c_1) = n\bar{c}_p, \quad \text{and} \quad S_n(c_1') = 0.$$

Hence we easily find

$$S_n(c_1'^2) = S_n(c_1^2) - n\bar{c}_p^2$$

$$S_m S_n(c_1'^2) = S_m S_n(c_1^2) - S_m(n\bar{c}_p^2),$$

* It might be considered better, if the standard deviations of the homologous parts vary very considerably with position, to measure the individuality by the ratio of this excess to the corresponding standard deviation. Not only, however, does the use of such a ratio immensely increase the arithmetical labour, which is a possibility, which of course, we could face, but there is also a question as to whether the ratio is really a *truer* measure of individuality. A full discussion of this important point must for the present be deferred.

Or, noting that

$$\bar{c} = S_m S_n(c_1)/(nm) = S_m(\bar{c}_p)/m, \text{ we have}$$

$$\sigma'^2 = \sigma^2 - \sigma_M^2,$$

where σ' is the standard deviation of the character-individualities free from the position factor. We see that it is precisely the same quantity as we have previously used for the mean standard deviation of the arrays for given positions.

Next taking the correlation of characters c'_1 and c'_2 in positions p_1 and p_2 we have

$$S_m(c_1'^2) + S_m(c_1'c_2') = S_m(c_1^2) + S_m(c_1c_2) - 2S_m(c_1)m\bar{c} + m^2\bar{c}^2.$$

To get this result we have multiplied every quantity like $c'_1 = c_1 - \bar{c}_{p_1}$ by every other quantity like $c'_2 = c_2 - \bar{c}_{p_2}$ *and by itself*, and then added such quantities together for every position on the one organism. Thus on the left hand side there are m terms in the first, $m(m-1)$ terms in the second summation; on the right hand side there are m terms in the first, $m(m-1)$ terms in the second and m terms in the third summation. Now sum for each of the n organisms, and we have

$$nm\sigma'^2 + nm(m-1)R\sigma'^2 = mn(\sigma^2 + \bar{c}^2) + nm(m-1)(\rho\sigma^2 + \bar{c}^2) - 2m^2n\bar{c}^2 + m^2n\bar{c}^2.$$

Whence

$$R = \rho\sigma^2/\sigma'^2 + \frac{\sigma^2 - \sigma'^2}{(m-1)\sigma'^2},$$

or, as before

$$R = \rho\frac{\sigma^2}{\sigma^2 - \sigma_M^2} + \frac{\sigma_M^2}{(m-1)(\sigma^2 - \sigma_M^2)} \quad \ldots\ldots\ldots\ldots \text{(x)}.$$

Now while this proof is independent of the theory of partial correlation coefficients, involving only simple algebra, and is further independent of any consideration of linear regression, it yet wants something of the width of the former theory, which allows us at once, for example, to correct for a combination of factors, such for example as for *both* growth and position influences simultaneously. The difficulty lies entirely in the extent within which it is legitimate to assume the relation between position or age, and the mean value of the character at that position or age to be linear. It is therefore clearly advisable to start by plotting this relationship,* and fitting, if possible, such position or growth graphs with appropriate curves. If, for the series of positions dealt with or the period of growth taken, we find that a straight line† is a close approximation to the relationship, then we

* In the case of some animals and many plants the relationship is in itself of much interest, for it expresses a law of development or growth in serial parts.

† The analytical consideration of this point is very simple. If the regression

may use the general theory of partial correlation, otherwise we must fall back on results like (x). For example, in head growth in boys, we cannot much improve on a straight line; in positional influence on the branches in the whorls of *Equisetum arvense* we need at least a third order parabola.

(8.) Although material for several investigations on the homotyposis of serial homologous parts has been collected, the progress in some of these cases is slow, as it involves rather laborious microscopic measurement. I content myself at present with an illustration from the vegetable kingdom.

I collected in the autumn of last year, 126 plants of *Equisetum arvense* in Raydale Side, an offshoot of Wensley Dale; the plant was growing on a lane side high up above Semmerwater. This *Equisetum* grows from the top with a single stem, and I counted the number of branches to the whorl from the root upwards. As a rule, there will be one or two whorls close to the soil which have never developed any branches at all; then we have what I shall term the *first* whorl in which some branches have developed, but the number is irregular and obviously subject to some cause of variation, other than the growth law of the plant. The number of branches to the whorl then increases uniformly and steadily up to the 4th whorl, after which it falls almost equally steadily to the 10th whorl. Beyond this the results becomes somewhat irregular again, for very few plants will be found—at any rate in the locality considered—with more than 12 or 13 whorls, and even in these whorls there is a certain amount of forking or irregularity which it is difficult to deal with. The plants were certainly fully developed

be linear, the means of the arrays all lie on the regression line, and the mean standard deviation of the arrays about their means is $\sigma\sqrt{1-r^2}$. If the regression be not linear, the means of the arrays will have a mean square deviation Σ_M^2 from the regression line. The mean square deviation of the arrays from the regression line, but *not from their means*, is still $\sigma^2(1-r^2)$. The mean standard deviation (deviation of mean square *from means*) is now given by

$$\sigma'^2 = \sigma^2(1-r^2)-(\sigma_M^2-r^2\sigma^2),$$

since $\sigma^2 = \sigma'^2+\sigma_M^2$. But we easily find

$$\Sigma_M^2 = \sigma_M^2-r^2\sigma^2.$$

Hence Σ_M is a good measurement of the deviation of regression from linearity, or of σ_M from $r\sigma$. If we take $\eta = \sigma_M/\sigma$, we have

$$\sigma'^2 = \sigma^2(1-\eta^2), \qquad \Sigma_M^2 = (\eta^2-r^2)\sigma^2.$$

Clearly η^2 must lie between r^2 and 1. Further, η can only vanish when the correlation is zero, or become ± 1 when the correlation is perfect. Between these values it gives the mean reduction in variability of an array as compared with the whole population. Further, the deviation of η from r is a good measure of the deviation of the system from linearity. Thus η is a useful constant which ought always to be given for non-linear systems. It measures the approach of the system not only to linearity but to a single valued relationship, *i.e.*, to a causal nexus.

when gathered at least as far as the 12th or 13th whorl, and I doubt whether even beyond this so late in the season, any further branching would have taken place. A few branches were broken off, and these were of course counted; there was no difficulty, however, in easily ascertaining whether a branch had in any case been developed or not, and the peculiarity of the 1st whorl was certainly not due to missing, but to undeveloped branches.

Table V gives the relation between branches to the whorl and position for the whole of the 126 plants. In two columns to the right are given the means and variabilities of the branches for each whorl.

Now, whether we judge by mean or standard deviation, we see a perfectly gradual change from whorl to whorl, which absolutely precludes us from considering the number of branches to the whorl as a pure homotypic character. We see a marked differentiation due to position of the whorl on the plant; the whorls are homologous but not homotypic parts.

Suppose, however, that we disregard our test for differentiation,* and proceed to find a correlation table for the whole material as homotypic. We have Table VI, for which I have to heartily thank Dr. Alice Lee.

The value found for the homotypic correlation from this table is

$$\rho = -0{\cdot}0064 \pm 0{\cdot}0185,$$

or, there is no sensible homotyposis at all.

But we might have gone to the other extreme and taken only the 3rd, 4th, and 5th whorls, which have more nearly the same means and standard deviations as homotypes. The result is Table VII, giving

$$\rho = 0{\cdot}7918 \pm 0{\cdot}0129.$$

It will be perfectly clear, therefore, as these two results ought to be the same, if the whorls were true homotypes, that we may get any result at all if we neglect differentiation.† The answer to this is that no trained biometrician would call these whorls "undifferentiated like organs" with the two right hand columns of Table V before him.

* On the test for differentiation, see 'Biometrika,' vol. 1, p. 334.

† Bateson, 'Roy. Soc. Proc.,' vol. 69, p. 200.

Table V.

Number of Branches to the Whorl.

Position of Whorl.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	Totals.	Mean.	Standard deviation.
1	2	2	3	10	9	8	13	29	22	17	11	—	—	126	7·619	2·360
2	—	—	—	—	1	3	5	21	37	40	16	3	—	126	9·294	1·273
3	—	—	—	—	—	—	9	9	35	45	23	5	—	126	9·627	1·187
4	—	—	—	—	—	—	6	10	33	45	28	3	1	126	9·730	1·151
5	—	—	—	—	—	—	8	10	35	41	30	2	—	126	9·643	1·158
6	—	—	—	—	2	3	6	13	35	38	24	3	—	124	9·427	1·363
7	—	—	1	4	2	6	12	23	28	29	17	1	—	123	8·732	1·781
8	2	3	7	5	5	13	21	23	24	14	4	—	—	121	7·297	2·291
9	8	10	13	14	9	14	19	17	10	5	—	—	—	119	5·555	2·553
10	18	20	13	11	17	14	11	5	1	—	—	—	—	110	3·964	2·199
11	31	29	18	9	5	3	1	1	—	—	—	—	—	97	2·443	1·506
12	24	34	6	2	—	—	1	—	—	—	—	—	—	67	1·866	0·960
13	24	14	—	—	1	—	—	—	—	—	—	—	—	39	1·462	0·746
14	8	4	—	—	—	—	—	—	—	—	—	—	—	12	1·333	0·471
15	3	1	—	—	—	—	—	—	—	—	—	—	—	4	1·250	0·433
16	2	—	—	—	—	—	—	—	—	—	—	—	—	2	1·000	0·000
Totals . . .	122	117	61	55	51	64	112	161	260	274	153	17	1	1448	—	—

Table VI.

Number of Branches on 1st Whorl of Pair.

Number of Branches on 2nd Whorl of Pair.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	Totals.
1	132	110	63	55	41	61	91	120	281	274	146	22	3	1399
2	110	98	49	49	57	54	90	129	281	237	160	18	1	1333
3	63	49	16	18	14	31	59	79	122	149	55	6	—	661
4	55	49	18	12	19	24	53	82	103	121	55	1	1	593
5	41	57	14	19	18	22	67	45	115	109	58	5	—	570
6	61	54	31	24	22	8	54	70	133	154	54	7	—	672
7	91	90	59	53	67	54	162	122	199	173	72	4	2	1148
8	120	129	79	82	45	70	122	300	281	262	111	28	2	1631
9	281	281	122	103	115	133	199	281	766	401	150	9	1	2842
10	274	237	149	121	109	154	173	262	401	914	240	25	2	3061
11	146	160	55	55	58	54	72	111	150	240	428	40	1	1570
12	22	18	6	1	5	7	4	28	9	25	40	34	—	199
13	3	1	—	1	—	—	2	2	1	2	1	—	—	13
Totals	1399	1333	661	593	570	672	1148	1631	2842	3061	1570	199	13	15692

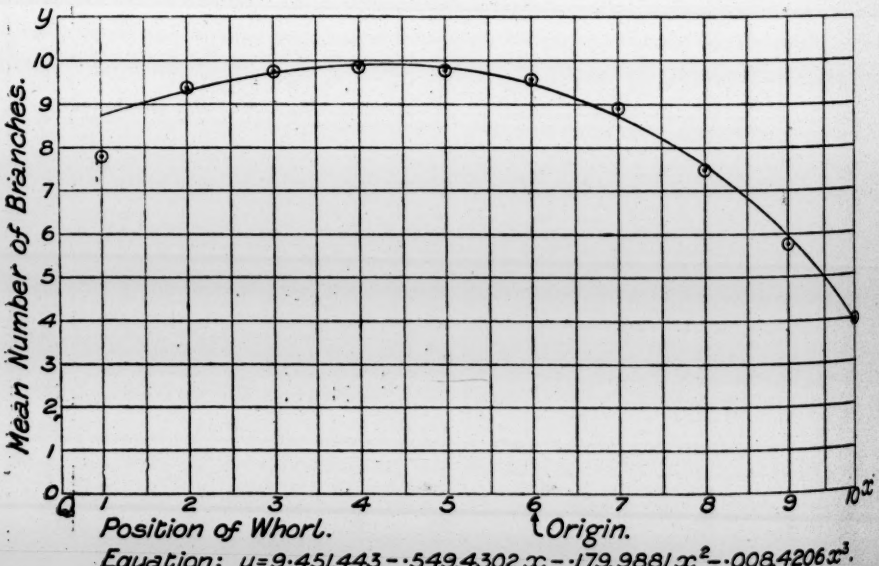
Table VII.—Whorls, 3rd, 4th, and 5th only.

Number of Branches to 1st Whorl of Pair.

Number of Branches on 2nd Whorl of Pair.	7.	8.	9.	10.	11.	12.	13.	Totals.
7	28	8	8	1	1	—	—	46
8	8	28	19	3	—	—	—	58
9	8	19	146	26	6	—	1	206
10	1	3	26	196	35	1	—	262
11	1	—	6	35	110	9	1	162
12	—	—	1	—	9	10	—	20
13	—	—	—	1	1	—	—	2
Totals	46	58	206	262	162	20	2	756

Now let us consider how to handle the material, allowing for the differentiation of the whorls. To begin with, our formula requires the use of the same number of homologous parts for each organism, and it is, on account of the value of the probable error of the random sample, undesirable to use fewer than 100 individuals. This leads to our cutting off Table V at the 10th whorl. In this way we get rid also of the forking, which certainly begins in many individuals at the 11th or 12th whorl. Table VIII gives us the data of Table V reconstituted for 110 plants, with ten whorls apiece. The only serious difficulty now remaining is that which I have referred to as arising from heterogenity in the first whorl. A glance at the mean and standard deviation of the branches in the first whorl given in Table V

Diagram 2.



Regression curve.—Whorl branches and position.

Table VIII.—Relation between Number of Branches and Position of Whorl.

Position of Whorl.	Number of Branches in Whorl.														
	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	Totals.	Means.
1	2	2	3	8	8	6	9	24	20	17	11	—	—	110	7·7182
2	—	—	—	—	1	3	4	16	32	38	13	3	—	110	9·3273
3	—	—	—	—	—	—	7	6	29	42	21	5	—	110	9·7182
4	—	—	—	—	—	—	4	7	27	43	25	3	1	110	9·8273
5	—	—	—	—	—	—	6	7	28	39	28	2	—	110	9·7455
6	—	—	—	—	1	3	5	7	31	37	23	3	—	110	9·5636
7	—	—	—	3	2	5	9	19	27	28	16	1	—	110	8·8909
8	1	2	5	5	5	11	19	20	24	14	4	—	—	110	7·4909
9	5	10	11	12	8	14	19	16	10	5	—	—	—	110	5·7364
10	18	20	13	11	17	14	11	5	1	—	—	—	—	110	3·9636
Totals 2—10	24	32	29	31	34	50	84	103	209	246	130	17	1	990	8·2515

will suffice to demonstrate this heterogeneity. Certain individuals have the normal number of about 8·5 branches to this whorl, but about a fifth of the total number of individuals only develop about half this normal number of branches. To illustrate this I have in Diagram 2 plotted the mean number of branches to the whorl, and fitted these means with a parabola of the third order,* using only whorls 2 to 10. The equation to this parabola is

$$y = 9{\cdot}451{,}443 - 0{\cdot}549{,}4302x - 0{\cdot}179{,}9881x^2 - 0{\cdot}008{,}4206x^3,$$

the origin being at the 6th whorl, and y giving the mean number of branches for x whorls from the 6th. We have the following results:—

Position of whorl.	Observed number of branches.	Calculated.
1	7·718	8·752
2	9·327	9·308
3	9·718	9·707
4	9·827	9·898
5	9·746	9·829
6	9·564	9·451
7	8·891	8·714
8	7·491	7·565
9	5·736	5·956
10	3·964	3·835

A much worse fit was obtained by striking a cubical parabola through all *ten* points.

It will be seen that the excellency of fit fully justifies the use of this curve. But that there is a large deviation from the observed mean of the 1st whorl, when we calculate its value from the curve thus obtained. Somewhat reluctantly, therefore, I felt compelled to omit the consideration of the 1st whorl from my investigations. Had I possessed a sufficient number of specimens I should have separated my material into two classes, those plants with normal 1st whorl and those with abnormal 1st whorl. But with my available material I should have had considerably less than 100 individuals to deal with, and accordingly I settled to take nine homologous parts only, namely, the 2nd to the 10th whorls, in which the differentiation appears to be solely due to position on the plant. Above the 10th whorl, the phenomenon of forking obscures the determination of branches to the whorl, while below the 2nd whorl the full or partial development of branches to the whorl seems to be determined by the local lower vegetation round the stem.

Taking Table VIII, I found for the mean of the means 8·2515 branches, and for the standard deviation of the means σ_M, σ_M^2 =

* By the method indicated in 'Biometrika,' vol. 2, p. 11.

3,938,354. Further, if σ be the standard deviation of the frequency distribution of branches, as found from the bottom row of Table VIII, we have

$$\sigma^2 = 6{\cdot}721{,}083.$$

Hence for use in formula (x) we have, since $m = 9$,

$$\frac{\sigma^2}{\sigma^2 - \sigma_M{}^2} = 2{\cdot}415{,}280, \quad \frac{1}{m-1}\frac{\sigma_M{}^2}{\sigma^2 - \sigma_M{}^2} = 0{\cdot}176{,}911 \ldots\ldots \text{(xi)}.$$

Table IX gives the uncorrected homotyposis for the nine whorls treated as simple homotypes. From this I find,

$$\rho = 0{\cdot}131{,}258 \ldots\ldots\ldots\ldots\ldots\ldots \text{(xii)}.$$

Substituting (xi) and (xii) in (x), we find for the homotyposis of the number of branches in the whorls in *Equisetum arvense*, when corrected for differentiation due to position,

$$\mathrm{R} = 0{\cdot}4939.$$

This result it must be admitted is extremely satisfactory, and indicates how it is quite possible to correct a result like (xii) by allowing for the differentiation of the homologous parts due to serial position.*

I hope before long to publish other results dealing with homotyposis in serial parts, where the differentiation has every variety of intensity. I think they will suffice to show that differentiation is not a subtle and evasive quality beyond the appreciation of the naturalist who is provided with the training requisite for modern biometric research.

(9.) The values of R as given by (ix) and (x) may be illustrated from the actual numbers for *Equisetum arvense*. We have seen in the footnote, p. 304, that

$$\eta = \sigma_M/\sigma.$$

This in our case gives

$$\eta = 0{\cdot}76549.$$

But by direct calculation on Table VIII, using whorls 2 to 10, Dr. Lee finds $r_{pc} = -0{\cdot}64616$. Hence with the notation of the footnote referred to

$$\sigma' = 0{\cdot}7632\,\sigma, \qquad \Sigma_M = 0{\cdot}4104\,\sigma.$$

* The value obtained for the crude homotyposis of the members of the whorls in *Asperula odorata* in my first memoir was $\rho = 0{\cdot}1733$ ('Phil. Trans.,' A, vol. 197, p. 326). I have little doubt that when we are able next summer to calculate the correction for differentiation in position of whorl, we shall find R for woodruff in good accordance with other homotypic results. My remarks about it were: "In counting the members on the whorls I soon found evidences of differentiation in position, the whorls towards the top of the spray having, as a rule, fewer members than those lower down" (*loc. cit.*, p. 325). Unfortunately I have not kept my records of position.

Table IX.—Uncorrected Homotyposis for *Equisetum arvense*.

Number of Branches in 1st Whorl of Pair.

Number of Branches in 2nd Whorl of Pair.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	Totals.
1	12	8	11	10	4	15	19	22	37	39	10	5	—	192
2	8	14	7	13	9	13	21	29	75	54	13	—	—	256
3	11	7	6	6	5	12	21	33	59	56	14	2	—	232
4	10	13	6	4	7	15	30	35	56	49	22	—	1	248
5	4	9	5	7	6	13	31	22	79	55	38	3	—	272
6	15	13	12	15	13	2	38	39	96	113	40	4	—	400
7	19	21	21	30	31	38	110	70	131	139	57	3	2	672
8	22	29	33	35	22	39	70	144	142	169	96	22	1	824
9	37	75	59	56	79	96	131	142	562	304	121	9	1	1672
10	39	54	56	49	55	113	139	169	304	770	197	21	2	1968
11	10	13	14	22	38	40	57	96	121	197	398	33	1	1040
12	5	—	2	—	3	4	3	22	9	21	33	34	—	136
13	—	—	—	1	—	—	2	1	1	2	1	—	—	8
Totals	192	256	232	248	272	400	672	824	1672	1968	1040	136	8	7920
Frequency of whorls	24	32	29	31	34	50	84	103	209	246	130	17	1	990

Thus η diverges much from r_{pc} and Σ_M from zero. Indeed, a glance at the diagram shows how far we are from true linear regression. If we use r_{pc} as above instead of η, *i.e.*, (ix) instead of (x), we have

$$R = 0{\cdot}3511,$$

a value very much below the actual value. This illustration will suffice to emphasise the importance of testing the actual curve of regression before we assume it to be linear and use equation (ix).

(10.) The subject of differentiation due either to position or age is, of course, a difficult one, but it does not seem at all beyond biometric treatment. The greatest difficulty which it seems to me will have to be encountered is not that of discovering and allowing for differentiation due to serial position, but in ensuring that when this has been allowed for, there is not remaining an organic correlation due to the necessity of adjacent parts "fitting." On this account it is most desirable that as large a number of homotypes as possible shall be taken, so that the part of the correlation due to the homologous parts having to fit, or, indeed, to serve a common end, should be reduced to as small a quantity as possible. For example, if we suppose adjacent whorls to have their number of branches influenced by an organic relationship, this result will only bias nine out of the forty-five pairs we should form in dealing with ten whorls. The question of separating organic from homotypic correlation is one that I hope to return to at a later date. Meanwhile the present paper will suffice to indicate how partial correlation coefficients enable the biometrician to free himself from the differentiation between individuals due to different periods of growth, or to different positions on the organism.

In conclusion I should like to thank Dr. Alice Lee and Mr. F. E. Lutz for aid readily granted at one or other stage of this investigation.

"Primitive Knot and Early Gastrulation Cavity co-existing with Independent Primitive Streak in *Ornithorhynchus*." By Professor J. T. WILSON, M.D., and J. P. HILL, D.Sc., University of Sydney. Communicated by Professor G. B. HOWES F.R.S. Received January 21,—Read February 12, 1903.

Amongst the material at our disposal for the investigation of the earlier stages in the development of the Monotremes is an egg of Ornithorhynchus, measuring 10 × 9·5 mm. The stage of development represented by this egg is of such interest and importance that we have deemed it deserving of a brief preliminary communication.

The stage of embryonic organisation would appear to fit in as immediately succeeding the oldest of the early embryos described by Semon,* viz., his "E_6" (fig. 15, Taf. 8). His "E_7" appears, indeed, to have been older than "E_6," but with the exception of one figure (fig. 39, Taf. 9) representing the structure of the extra-embryonic blastoderm, Semon gives no indication of the conditions met with in that ovum. This is the more to be regretted, as we see reason to believe that "E_7" must have corresponded pretty closely with the stage now to be described. Semon notes that the embryonic area of this egg was injured through an unfortunate accident.

The general structural arrangements of the Monotreme ovum in its early stages of development have been described and figured by Semon. In all the stages dealt with by him, illustrating the development from segmentation up to the first indications of gastrulation, the yolk had retained its original arrangement as a tolerably coherent solid or semi-solid spheroidal mass with alternating strata of white and yellow yolk-spheres. As he states, however, the egg in the course of further development increases considerably in size during its sojourn in the uterus through absorption of fluid. By this process of fluid absorption, the yolk-mass is disintegrated and its spherules disseminated throughout the interior of the growing blastodermic vesicle, though many of them remain adherent to the deep surface of the blastodermic membrane.

This conversion of the solid or semi-solid yolk-mass into the fluid contents of a large blastodermic vesicle renders the investigation of the structure of the blastoderm, from the period of the commencement of gastrulation up to the formation of a distinct embryo, an exceedingly difficult one. The loss of Semon's "E_7" may very likely be attributable to such difficulties in the way of manipulation as we have encountered in dealing with a delicate blastodermic membrane surrounded by a thick, tough, and opaque shell as well as a vitelline

* Semon, R., 'Zool. Forschungsreisen,' &c., Bd. 2, Lief. 1 (1894).

membrane, and distended by a considerable bulk of fluid material. It is quite impossible to remove the shell without serious damage to the delicate blastoderm. Fixation with the shell intact is imperative, and, as a matter of fact, the result in the way of preservation proves quite satisfactory, as evidenced by the condition of the cellular blastoderm, in which mitotic figures are well preserved. But even after fixation, the opening up of the ovum is attended with no little risk. The inevitable evacuation of the contained fluid allows of crumpling of the blastodermic membrane with possibility of injury to the embryonic area. It is impossible, owing to its size and osmotic difficulties, to treat the ovum throughout unopened. Even were that course practicable, the impossibility of orientation would be an insuperable difficulty.

After fixation and subsequent dehydration in graded alcohols, the 10 mm. egg was cleared in origanum oil and opened. The blastoderm still remaining *in situ* in relation to the shell was examined from the interior aspect, and was found to possess at one spot a small more opaque area, somewhat oblong, but rather irregular. The portion of the blastoderm containing this small opaque patch was photographed by transmitted light at a magnification of 6·5 diameters for the purpose of orientation.

Our surmise that the area in question was of the nature of an embryonic or primitive knot was afterwards confirmed by the examination of serial sections.

No differentiation in way of an embryonic area in the wider sense is recognisable in the photograph, nor was any such discovered in the course of examination of the wall of the blastodermic vesicle *in toto* under low magnification. We naturally concluded that the very evident knot represented the earliest and only differentiated area, and for a time devoted our attention solely to this area and the blastoderm in its vicinity. The portion of the blastoderm containing the knot was separated from the remainder of the wall of the vesicle, and was then imbedded and cut in serial sections. Examination of these did in fact show that in the neighbourhood of the knot, and for some distance from it, the wall of the vesicle was destitute of any indication of further differentiation. But towards the periphery of the portion sectioned and comparatively remote (nearly 2 mm.) from the knot, itself, we found the commencement of a region of thickened ectoderm with underlying mesoderm. Our attention thus being directed to other manifestations of developmental activity in the blastoderm in addition to the primitive knot, we found, in the portion of the vesicular wall originally put aside, a quite extensive area showing important changes. These amount to no less than the establishment, quite away from the region of the knot, of a distinct linear primitive streak formation, surrounded by an area over which the ectodermal

layer is thickened and cubical, and within which a mesodermal sheet has already undergone a wide extension.

It is peculiarly unfortunate that, owing partly to the difficulty of dealing with the delicate and originally collapsed and torn vesicular wall after the cutting open of the egg, partly to the failure to detect any visible differentiation apart from the knot, and our consequent conviction that the latter was the sole trace of embryonic organisation yet present, the orientation of the distinct portions into which the vesicle was separated was not adequately determined and preserved. It thus became impossible for us to guarantee that the planes of sectioning of the remaining portions should be accurately co-ordinated with each other, or with that of the important piece first sectioned. This failure, not wholly blameworthy, when the conditions of the task are appreciated, has introduced an element of conjecture into our subsequent attempt to determine the precise relation to the knot of the primitive streak area, of whose existence we later on became aware. Nevertheless, we think that we shall be able to establish these relations with at least a high degree of probability.

In this preliminary paper we propose to restrict ourselves to an account of the highly interesting area which first attracted our attention, and which we have already referred to as a "primitive knot." It will, we think, be admitted that no possible doubt can be entertained of the justice of employing for its designation a term which would stamp it as the homologue of the well-known structure in many Sauropsida to which the same name has been applied. It is a true primitive or gastrula knot, in the Sauropsidan sense, possessing a transversely elongated gastrula-mouth or blastopore and an invagination cavity, which both in appearance and minute structure resembles in the closest manner the structure described under these names in various Reptilian forms.

But if this be admitted, we come face to face with this extraordinary fact that, in addition to this reptilian-like gastrula, there is to be found in Ornithorhynchus, quite distinct and even remote from this gastrula knot, a region in which there is being differentiated independently a primitive streak of quite ordinary and typical mammalian character.

The oblong portion of the wall of the vesicle containing the primitive knot near its centre, and measuring about 8 × 10 mm., was left adhering to the portion of the shell covering it. The direct observation and photography of the piece were thus confined to its deep aspect so as to avoid undue disturbance and injury. The whole piece was then double imbedded in photoxylin and paraffin and cut into serial sections 10 micra thick. The sections were stained in hæmatëin and eosin.

The plane of section chosen was approximately at right angles to

the larger diameter of the embryonic knot, and parallel to its presumable axis, on the supposition that a clear spot near one of its margins represented the position of the gastrula opening or blastopore, thus determining the true posterior margin of the knot.

Examination of the series did ultimately show that the sections were approximately longitudinal sections through the gastrula cavity.

The general character of the egg at the stage under consideration must be briefly referred to. At the period now dealt with, the formerly yolk-laden ovum has become transformed into a large blastodermic vesicle with fluid contents, amongst which are large numbers of dispersed yolk-spheres of the original yolk-mass. One can, without hesitation, homologise the interior of the vesicle with the subgerminal cavity of a Sauropsidan egg, extended so as to include by liquefaction the whole of the yolk itself. Ornithorhynchus indeed may be said to afford an actual demonstration of the transformation of a Sauropsidan subgerminal cavity, such as is figured in its first beginnings in Semon's figures 36 and 38 of his "O_3," into the cavity of a mammalian blastodermic vesicle, thus supporting Keibel's view of the correspondence of these cavities.

The establishment of the vesicular stage has been effected by the extension of the cellular blastoderm completely around the yolk, a condition representing a considerable advance on that existing in Semon's figures "E_5" and "O_3." He does not record the condition in this respect of his "E_6." He was also unable fully to determine the condition of "E_7" in this respect. We believe that the latter must have been very similar to our present stage, judging from the figure he gives of a portion of the extra-embryonic blastoderm.

Not only is the cellular wall of the blastodermic vesicle complete in our specimen, but it is already bilaminar throughout, and trilaminar over a not inconsiderable area. A complete layer of yolk-entoderm ("secondary" entoderm) has been differentiated lining the cavity, except under the small area of the primitive knot, with whose tissue it is continuous. The yolk-entoderm cells are large and swollen, being distended by yolk-spheres of various sizes and somewhat different staining capacities. Owing to their yolk-laden character their protoplasmic contents are relatively greatly reduced. The nuclei are, as a rule, large and vesicular. In the least successful sections the entoderm cells may be broken up and more difficult of recognition. We are firmly convinced that it is owing to the imperfection of the section shown in Semon's figure 39 from his "E_7" that such a careful observer has been unable to recognise the yolk-entoderm as such, and figures it as a zone of vacuolated coagulum next the surface of the yolk. The imperfect layer of angular-looking cells which he figures and regards as the entoderm in "E_7" is not really such, but forms an intermediate layer of mesodermal cells.

This layer of mesoderm is well represented in the stage under description. But it by no means extends throughout the whole extent of the blastodermic vesicle, nor is it met with at all at or in the immediate vicinity of the primitive knot. It is found most fully developed in and around the primitive streak area, and here it is continuous with the ectodermal thickening of the primitive streak (*i.e.*, paraxially), where it attains greater thickness and consists of several layers of cells. Outwards from the line of the primitive streak* it thins out gradually into a single layer, and further out becomes patchy and incomplete. It is throughout distinct and independent of the underlying yolk-entoderm, which can be followed through the whole wall of the vesicle. We have several preparations of small portions of the wall of the vesicle stained and mounted *in toto*, which give clear demonstration of the huge yolk-laden entoderm cells forming a continuous lining of the vesicle wall, precisely as figured by Hill and Martin† in a considerably later stage.

The ectoderm of the vesicle wall throughout the greater part of its extent forms a membrane consisting of very thin flattened cells closely applied to the vitelline membrane. This character it retains in the vicinity and over a portion of the primitive knot. But over the region already referred to as the primitive streak area, remote from (posterior to) the primitive knot and extending outwards so as to be practically co-extensive with the mesoderm of this area, the ectoderm shows a marked change in character. Here its cells are no longer flattened and squamous, but thickened and cubical, and here also their developmental activity is often betrayed by the existence of frequent mitotic figures. In the posterior part of the primitive streak thickening, the cubical ectoderm gradually becomes more attenuated, the mesoderm still continuing beneath it for some distance, even after the "extra-embryonic" character of the ectoderm is assumed.

We now come to the "primitive knot" itself, whose occurrence and characters it is the special object of this brief paper to record.

The oblong area of the blastoderm constituting the knot measured 0·42 mm. in one diameter (antero-posterior axis of gastrula) and 0·49 mm. in the other diameter (transverse). It is therefore comparable in general form with the area from an Echidna egg (E_6) figured by Semon in his fig. 15 (*loc. cit.*). Unfortunately Semon gives no clue to the magnification of this figure, which is on a different scale to the other illustrations. One cannot therefore compare the area of E_6

* The plane of section through the primitive streak area of the vesicle wall, though its orientation was determined in rather haphazard fashion in default of any visible guide, is obviously approximately transverse to the axis of the primitive streak, showing the characteristic bilateral symmetry of organisation of this important region.

† Hill and Martin, "On a Platypus Embryo from the Intra-uterine Egg," 'Proc. Linn. Soc., N.S.W.,' vol. 10, figs. 31—33.

in size with that now described. It is to be regretted that Semon's specimen was lost through accident, so that no further comparison can be instituted. There is, however, a general resemblance between fig. 15 of Semon's E_6 and his figs. 14 and 18 showing areas from other eggs of Echidna (E_5) and Ornithorhynchus (O_3). The dimensions of these can be calculated and are both found somewhat to exceed those of our primitive knot, in spite of their relatively younger ages. It is therefore difficult to say to what extent the areas referred to (of E_6 and E_5 and O_3) correspond to our primitive knot. We are, however, of opinion, that E_6, though probably younger, must have pretty closely corresponded, and there can be little doubt but that the centre of the other areas, at least, is later on developed into an actual primitive knot. The sectional figure of O_3 shown in fig. 38, interpreted by Semon as showing the commencement of gastrulation, may possibly bear this interpretation, but we cannot feel satisfied that the granular coagulum layer between the cellular layer and the surface of the yolk shown in this figure has been adequately interpreted, especially in view of our complete conviction that in fig. 39 the layer represented in somewhat similar fashion is beyond all doubt really yolk-entoderm.

We do not wish to be taken as suggesting that here in O_3 the coagulum represents differentiated yolk-entoderm. We are simply not convinced beyond all doubt that all the formed cell elements of this area in O_3 are disposed in the form of a superficial continuous membrane as shown in fig. 38, more especially in view of the totally different condition of the nearly related stage E_5, as illustrated in the sectional fig. 33, and of the condition described below in our own somewhat later stage in Ornithorhynchus.

The general form of the embryonic knot has already been noted. A schematic surface projection of its outline is shown in fig. 1. This

FIG. 1.

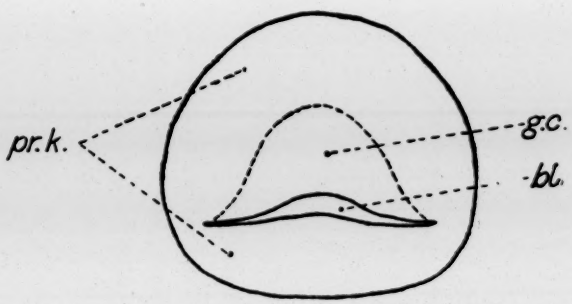
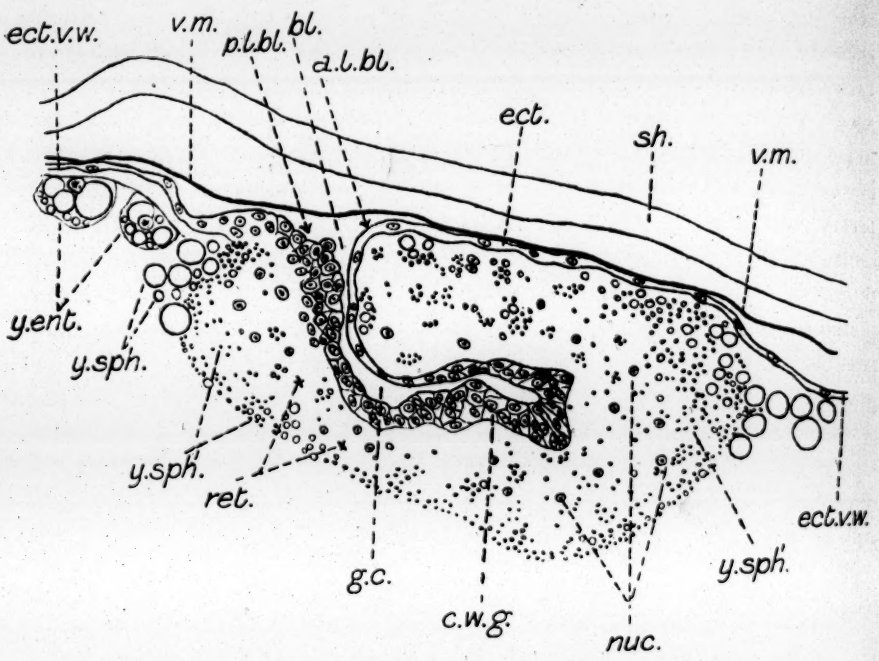


figure represents a plane reconstruction of the area of the knot, compiled from the serial antero-posterior sections through the region. One of the more typical of these sections is also figured semidiagra-

matically in fig. 2. More adequate illustration by photomicrography and otherwise will be forthcoming in a future communication.

Fig. 2.



In mesial section, the knot is seen to form a thick and prominent lenticular mass projecting into the cavity of the blastodermic vesicle. It is largely composed of a loose reticular tissue in which nuclei are only sparsely distributed and cell outlines are for the most part invisible. This tissue is thickly dotted with minute yolk-spherules and small vacuoles, and is not limited towards the cavity of the vesicle by any very sharp or clear-cut boundary. This reticulum of the knot is continuous peripherally with the yolk-entoderm of the bilaminar blastoderm around the knot.

Penetrating the interior of the knot is the archenteric or gastrula-cavity, opening on the surface at the blastoporic aperture near the hinder part of the knot and appearing in sagittal section as a curved canal passing from the blastopore at first deeply, and then forwards, to end blindly in the more anterior part of the knot. This cavity is lined throughout by a very definite cellular wall.

Both in front of and behind the knot, the blastoderm is simply bilaminar, with thin ectoderm closely applied to the deep surface of the vitelline membrane. The entodermal cells are large and contain yolk-spherules of varying size and staining reaction and loose yolk-spheres are also found adherent to its deep surface.

The thin ectoderm is continued over the knot from the region in

front without change of character as far as the transversely elongated blastopore seen in fig. 1 and in section in fig. 2. Thence it is continued round the anterior lip of the blastopore so as to line the roof of the blastoporic (archenteric) canal or gastrula-cavity.

Posteriorly to the knot the thin ectoderm is continued forwards, but on reaching the posterior edge of the knot, its cells lose their markedly flattened character, and become continuous with a mass of cuboidal or even rounded cells, which forms the prominence of the posterior lip of the blastopore. The superficial cells of this mass do not, however, wholly surrender their epithelial arrangement, and are continued forwards in the hinder wall and floor of the gastrula-cavity to complete the cellular lining of this cavity. This cellular lining consists, for the greater part of its extent, of large and cuboidal cells, but in the hinder part of the roof, and extending from this upwards around the anterior lip of the blastopore, the cells are more flattened, ultimately becoming continuous with the thin ectoderm overlying the anterior part of the knot.

Figs. 1 and 2 together enable one to realise the form of the gastrula-cavity as a wide, but dorso-ventrally flattened cavity opening posteriorly by a transversely-elongated blastoporic aperture.

The posterior lip of the blastopore is formed by a mass of cells which, if not wholly indifferent, at least show a less clear differentiation into superficial and deep than is found elsewhere.

As was indicated at the outset, the resemblance between the primitive knot in Ornithorhynchus and that found in a number of reptilian forms is very striking. We would particularly draw attention to the figures by Mitsukuri* of the primitive knot in Chelonia, and more especially to his figs. 9 and 13 on Plate 8, which show a remarkable similarity to the condition here described, in all essential features. In Ornithorhynchus we lack entirely the columnar arrangement of the ectoderm over the knot, and the anterior lip of the blastopore there shown, otherwise the characters both of the knot and of the gastrula-cavity and its wall appear to be almost identical with those figured, especially in Mitsukuri's fig. 9, Plate 8.

In a future more extended communication we hope to illustrate more adequately the points above set forth, and in addition to add something in the way of elucidation of the fate of the primitive knot in somewhat later stages. We propose also to describe and illustrate the condition of the primitive streak area, which at the period now dealt with already co-exists with, though independently of, the primitive knot, but which later by extension comes into more intimate relationship with the knot.

* K. Mitsukuri, "On the Process of Gastrulation in the Chelonia," 'Journ. Coll. Sci., Japan,' vol. 6. *Cf.* also Gertrude C. Davenport, 'Radcliffe Coll. Monograph,' No. 8. Boston, 1896. Dendy, 'Qu. Jl. Micro., Sci.,' vol. 42, p. 18, 1899.

The mere fact of its co-existence at this stage with the knot, necessarily occasions some reconsideration of the morphological relationship of the mammalian primitive streak to the process of gastrulation.

"The Brain of the Archæoceti."* By G. ELLIOT SMITH, M.A., M.D., Fellow of St. John's College, Cambridge, Professor of Anatomy, Egyptian Government School of Medicine, Cairo. Communicated by Professor G. B. HOWES, LL.D., D.Sc., F.R.S. Received January 15,—Read February 12, 1903.

So far as I have been able to ascertain, nothing whatever is known of the form of the brain or, more strictly, of the cranial cavity in the Archæoceti. Hence no apology is needed for presenting even this imperfect account of two cranial casts representative of this sub-order, which have come into my hands.

Among the Eocene remains found in the Fayûm region of the Egyptian desert by Mr. H. J. L. Beadnell and Dr. Charles W. Andrews, in 1901, there was a broken skull of *Zeuglodon*,† from which it was possible to obtain a mould, representing the form of the greater part of the dorsal and lateral aspects of the brain. A plaster cast was made in the British Museum at the instance of Dr. Andrews, who kindly placed it at my disposal for description.

In the following winter (1902), Mr. Beadnell found in the same locality a natural cranial cast of the same size and general form as the artificial cast of *Zeuglodon*. It is obvious at a glance, if the two specimens be placed side by side, that the natural mould belongs to some member of the Archæoceti, but whether to the same species or even genus as the other specimen must at present remain an open question.

Mr. Beadnell kindly placed this specimen at my disposal.

The size and relative proportions of the different parts are almost identical in the two casts. Nevertheless, there are a considerable number of differences, some features being displayed in one and not in the other, and *vice versâ*. Many of these differences are obviously due to the imperfections of the casts, and especially to the failure of the plaster mould to represent the true form of the brain. But there are

* These notes were originally intended for the Report on the Survey of the Fayûm, to be issued by the Egyptian Survey Department, and are now published separately with the permission of the Under Secretary of State for Public Works and Captain H. G. Lyons, Director-General of the Survey Department.

† C. W. Andrews, "Extinct Vertebrates from Egypt," Part II. (Extracted from the 'Geological Magazine,' N.S., Decade IV, vol. 8, 1901, p. 437,—*Zeuglodon Osiris*, Dames'.)

several peculiarities of the natural cast—such, for example, as the form of the caudal part of the cerebellum and the shape of the cerebral hemispheres—which are difficult to reconcile with the artificial mould, even if we admit that the inner face of the cranium (from which the latter was made) is damaged or imperfectly cleaned. The differences, nevertheless, are sufficiently pronounced to indicate a generic distinction between the two specimens; and in this connection it is interesting to recall a statement made by Dr. Andrews in his first reference to *Zeuglodon*, as "including apparently Dames' *Z. Osiris*, and perhaps a second species."* It would, however, be very unwise, because it would serve no useful purpose, and possibly lead to error, to found a new genus or even a new species on the evidence of this natural cranial cast, when our source of information concerning the known genus (*Zeuglodon*) is as unsatisfactory as that obtainable from the artificial one about to be described. More especially so, when it is remembered that in the case of the only indisputable facts (*i.e.*, size and general form) the two casts are agreed. I shall therefore merely describe and attempt to explain the meaning of the form of the two specimens, and leave the question of the specific identity open for future research.

The general appearance of the brain is extraordinarily peculiar (figs. 1 and 2). The shape of the anterior part of the natural cast (fig. 1, *a* and *b*) closely resembles the cerebrum of a Lizard greatly magnified. An anterior prismatic stalk (*a*), representing the pedunculi olfactorii, suddenly expands into a plump, broad, smooth mass (*b*), showing the form of the chief part of the cerebrum. The maximum breadth of the two hemispheres (fig. 1, *b*) is 95 mm.; the greatest length of each (measured in front from the point where the ventral surface of the olfactory peduncles appear to expand into the chief mass of the hemisphere) is 47 mm.; and the maximum depth is 54 mm. Each cerebral hemisphere (exclusive of the olfactory peduncle) is slightly broader than it is long.

The two olfactory peduncles are represented in the natural cast by a single prismatic process. This extends forward for a distance of 37 mm. (measured along the dorsal edge) in front of the point where the expansion to form the hemispheres commences; and as the peduncles are broken across there, it is not possible to estimate their total length or the shape and size of the olfactory bulbs.

The coronal section formed by its anterior (broken) surface gives an isosceles triangle with a base measuring 8·5 mm. and sides of 10 mm. each. It expands as it passes backward, so that at its junction with the rest of the hemisphere its sides are each 19 mm. and its base 16 mm. in length.

In the artificial cast (fig. 2) all that represents this extensive

* 'Geological Magazine,' September, 1901, p. 401.

olfactory stalk is an irregular rostrum with two small boss-like projections, one above the other (*a* and *a'*). The cerebral hemispheres in the natural cast have a broad base, from which the sides extend upward toward the narrow dorsal surface with a gradual slope. In the artificial cast, however, the lateral parts of the hemispheres seem to be expanded into full rounded swellings.

Then, again, the antero-posterior diameter of the hemisphere is much shorter (being about 13 mm. less) than it is in the natural cast, although the breadth of the two specimens is approximately the same.

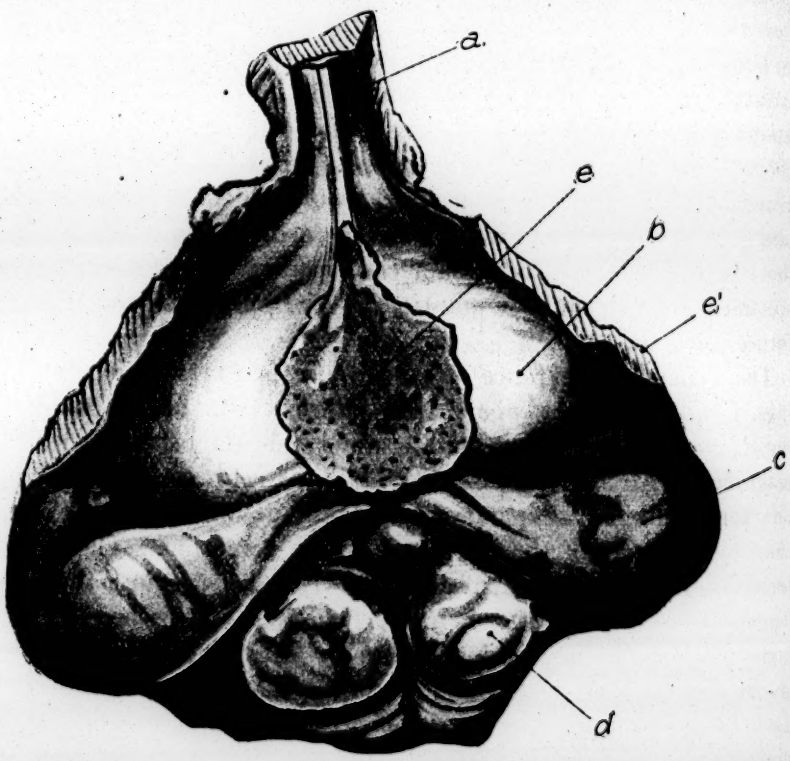


FIG. 1.—Dorsal aspect of the natural cast described in the text. $\frac{2}{3}$ natural size. *a*, olfactory peduncles; *b*, cerebral hemisphere; *c*, *d*, cerebellum; *e*, *e'*, fragments of skull.

It may be that the anterior parts of the skull, from which the artificial cast was made, are so damaged that little reliance can be placed upon the mould as an indication of the exact form of the brain. In fact, if this artificial cast even approximates to the form of the brain, it is quite certain that it did not belong to the same genus as the animal from which the natural cast was derived.

In other words, as we know that the artificial cast belonged to *Zeuglodon*, the probability is that the natural cast furnishes the first evidence of some hitherto undescribed genus of Archæoceti.

Behind the part *b*, which I have just described as the cerebrum,

there is (in the natural cast) a large, irregular mass of a very peculiar shape, not exactly comparable to the condition occurring in any other brain known to me.

Immediately behind the hemispheres (*b*) there is a great transverse bar (*c*) measuring 125 mm. in the transverse direction—*i.e.*, extending on each side 15 mm. beyond the lateral margin of the cerebrum (*b*).

Each lateral extremity of this mass (*c*) is expanded to form a large buttress. In the natural cast these buttress-like masses are practically

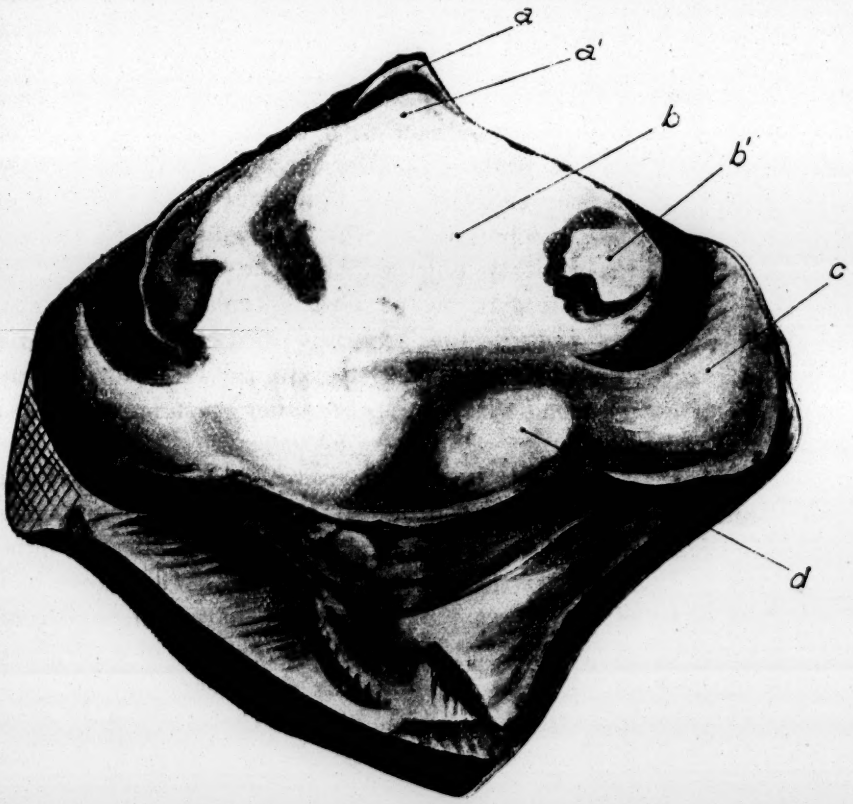


FIG. 2.—Dorsal aspect of the artificial cranial cast of *Zeuglodon*. $\frac{2}{5}$ natural size. *a*, *b*, *c*, *d*, as in fig. 1. *a'*, the dorsal rostrum, and *b'*, an irregular boss on the cerebral hemisphere. (These are probably due to imperfections in the cranium.)

vertical, and of uniform thickness; whereas in the artificial cast they are obliquely-placed, and expanded ventrally. In the natural cast the mesial continuation of these thick lateral masses (each of which measures 30 mm. antero-posteriorly) becomes reduced to a bridge measuring only 5 or 6 mm. [the exact figure cannot be stated, because a piece of bone (fig. 1, *e*) partially covers this region].

In the deep concavity behind the narrow bridge of the area *c* (in the natural cast) two rounded, irregular, walnut-like bosses project, one on each side of the middle line (fig. 1, *d*). Each of these is 26 mm. in

diameter, and is placed so obliquely that its surface looks almost directly backward. Shallow but clearly defined furrows separate these two bodies from each other and from the area *c*. In the artificial cast there is only a very faintly-marked indication of these bodies (fig. 2, *d*).

At a first glance it might seem that they represent the whole cerebellum, in which case *c* would be part of the cerebrum! But careful examination of the natural cast renders such an interpretation highly improbable, and comparison with the artificial cast seems to finally establish the belief that the whole of the region marked *c* forms part of the cerebellum.

It is extraordinarily difficult to accurately interpret this peculiar form of cerebellum. A comparison with other primitive types of cerebellum* points to the probability that the lateral buttresses of the mass *c* represent the floccular lobes, and that the walnut-like mass (*d*) represents the cerebellar lobule which I have called "area C" (*op. cit.*, 'Catalogue,' p. 211). If it be objected that the lateral buttress-like mass is much too extensive to be entirely "floccular," attention may be called to the fact that in the large aquatic Sirenia, which have retained an exceedingly primitive type of brain, the floccular lobes are enormous in comparison with those of other mammals (*op. cit.*, 'Catalogue,' p. 346).

It would perhaps be difficult to find elsewhere in the mammalia a greater contrast than that presented by the smooth, reptilian-like cerebral hemispheres of these casts and the highly complicated, ultra-mammalian neopallium of the recent whales, both Odontoceti and Mystacoceti.† And yet, if we inquire into the nature of the factors which have moulded the form and determined the size of the various parts of the brain in Eocene times and at the present, the contrast between the brain of *Zeuglodon* and the modern Cetacea loses much of its significance, and becomes much less peculiar, even though it may not be wholly explained.

In most Eocene mammals the cerebral hemispheres were exceedingly diminutive in comparison with those of their modern descendants and successors. Moreover, the bulk of the primitive mammalian hemisphere was composed of those parts (hippocampus and lobus pyriformis), which are pre-eminently olfactory: in other words, the neopallium (*i.e.*, that part of the pallium which is neither hippocampus nor pyriform lobe) is especially insignificant. It is a well-known fact that the sense of smell loses much of its importance in mammals of aquatic habits (*e.g.*, *Ornithorhynchus*, the Sirenia, the Pinnipedia, and especially the Cetacea), and in these animals the olfactory parts of the

* Compare, for example ('Catalogue of the Royal College of Surgeons,' 2nd edition, vol. 2, 1902), Armadillo (p. 211), Tapir (p. 311), Manatee (p. 346).

† *Vide* 'Catalogue of the Royal College of Surgeons,' *op. cit.*, pp. 348—359.

brain dwindle to very small proportions. In the Odontoceti the olfactory bulb and its peduncle actually disappear. The Archæoceti, therefore, are subject to two factors, which will account in some measure for their small cerebrum. For, in addition to the smallness of the brain to which most Eocene mammals are subject, there is their aquatic mode of life. This causes a reduction in size of just those portions of the pallium which form the greater part of the Eocene hemispheres.

In the modern Cetacea the neopallium attains to the greatest absolute size which it ever reaches in any mammal. This fact cannot, however, be considered fatal to the belief in the close affinity of the Archæoceti and the Cetacea, because the extraordinary dissimilarity between the brains in the two sub-orders is such as we know to have been produced by the operation of well-recognised causes in the long lapse of time which separates the dawn of the Tertiary period from the present day. In all mammals which lead a life "in the open" it has become a condition of their survival that the neopallium must increase in size in each successive generation: failing this, the creature must either adopt a "retired and safe mode of life" or become extinct. Numerous examples might be quoted in support of this hypothesis. But the case of the Sirenia shows us how little we really know of the factors which determine the size of the brain. These creatures began the struggle for existence in Eocene times with relatively large brains, in spite of their aquatic mode of life; and they have been succeeded by generations of descendants whose latest progeny at the present day have a brain-equipment only slightly superior to their earlier Tertiary ancestors (*vide* 'Catalogue,' *op. cit.*, p. 344, *et seq.*). Even if we admit that the modern Manatees and Dugongs lead an eminently safe and retired life, which is in marked contrast to the venturesome and "open" life of the whales and porpoises, much still remains to be satisfactorily explained.

Perhaps the most striking feature of the brain of *Zeuglodon* is the extreme disproportion between the size of the enormous cerebellum and the diminutive cerebrum. In this respect the fossil brain presents a most marked contrast to that of all recent mammals, and especially to that of the Cetacea. This relatively great size of the cerebellum is not peculiar to the Archæoceti, but is common to other extinct mammals of large size. In my memoir on the brain in the Edentata* the difficulty presented itself of adequately explaining a similar phenomenon in *Glyptodon*; and it must be borne in mind, in even attempting to do this, (1) that the obtrusive greatness of the cerebellum presents itself only in large mammals and not in lowlier vertebrates, and (2) that the size of the cerebellum is not proportionate

* "The Brain in the Edentata," 'Linnean Society's Trans.,' 2nd series, Zoology, vol. 7, part 7, 1899, p. 381.

to that of the cerebrum. In the case of *Glyptodon* I four years ago attempted to explain these facts in this manner.

The development of the neopallium in mammals opens up the possibility of the performance of many more complex muscular acts than are possible in the Amphibia or Reptilia: these acts require a co-ordinating mechanism, the size of which will be largely determined by the bulk of the muscular masses, the actions of which are to be harmonised, and the extent of the sensory surfaces which send into the cerebellum streams of controlling impulses. A large cerebellum is being demanded by a large mammalian body, even if the cerebrum is small. I cannot offer any more satisfactory explanation of the magnitude of the cerebellum in *Zeuglodon* than this.

It is clear from the foregoing that the extraordinarily great contrast in the appearance of the brain of the Archæoceti and that of the Cetacea cannot be urged as a reason against their kinship, when it is remembered that the operation of known factors is quite sufficient to explain the transformation of the one type into the other in the time which has separated the Eocene period from the present.

Having disposed of these negative arguments, we may consider the positive evidence for Cetacean affinity in the brain of *Zeuglodon*.

The shape of the cerebrum, and especially its relatively great breadth, is peculiar. In fact, this form of hemisphere rarely or never occurs among mammals, other than the Cetacea. I have elsewhere* attempted to explain the shortness of the Cetacean hemispheres by the fact that the abortion of the basal (olfactory) parts of the cerebrum limits their longitudinal extension. This, however, is not the whole explanation, because in many microsmatic Sirenia (*Halicore*), and Pinnipedia (*Otaria*, *Phoca*) the hemispheres are not especially broad. The disproportionate breadth seems, in fact, to be to some extent a characteristic of the Cetacea; and, in this respect, *Zeuglodon* agrees with them.

The peculiar elongation of the olfactory peduncles beyond the anterior extremities of the hemispheres is rarely found in mammals, though it is common enough in Reptiles and the Ichthyopsida. In fact, the exact parallel to the condition found in *Zeuglodon* occurs among recent mammals only in the Cetacea.† An analogous condition is found in the extinct Lemuroid *Megaladapis* [described by Forsyth Major (*op. cit.*)] and some *Amblyopoda*.

It is not without interest to note that the two outstanding features of the cerebral hemispheres of the Archæoceti, even if their value as indices of kinship be slight, both find their nearest parallel in the

* 'Catalogue of the College of Surgeons,' *op. cit.*, p. 350.

† Full references to this are given by Forsyth Major, "On the Brains of Two Sub-fossil Malagasy Lemuroids," 'Roy. Soc. Proc.,' vol. 62, 1897, p. 48, second footnote.

Cetacea. There are no characters of the brain of the modern Cetacea which can be regarded as certainly distinctive, if we put aside such features as the extreme dwindling of the olfactory apparatus, and the enormous development of the neopallium. Both must be regarded as late acquisitions, not to be expected in an Eocene mammal. Under these circumstances these slight points of positive evidence of the relationship of the Archæoceti and Cetacea must be allowed some value, as reinforcing the testimony of the skeletal parts.

If we seek to institute closer comparisons between the brain of *Zeuglodon* and of the Odontoceti and Mystacoceti with a view to the determination of its relationships, we are not unnaturally doomed to disappointment. It might, perhaps, be supposed by some anatomists that the absence of an olfactory bulb in the Odontoceti might point to a closer affinity of *Zeuglodon* to the Mystacoceti, in which a small olfactory apparatus is retained. But there is every indication that the olfactory apparatus of the Odontoceti has become aborted quite recently.

Thus in a specimen of the embryonic brain of the Narwhal (*Monodon*), which was given to me some years ago by Professor Howes, the remains of the olfactory bulb (fig. 3, *b.o.*) are still quite visible as a small umbili-

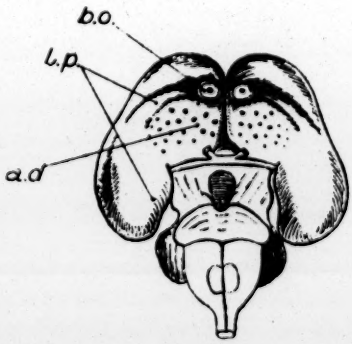


Fig. 3.—Ventral aspect of brain of an early fœtus of *Monodon*. Natural size. *a.d.*, locus perforatus (desert region); *b.o.*, bulbus olfactorius; *l.p.*, lobus pyriformis.

cate area in part of the "desert region" of Broca (fig. 3, *a.d.*), wherefore it follows that in the early embryo the olfactory bulb and peduncle develop as in all other mammals. Moreover, in all Odontoceti traces of the pyriform lobe are found even in the adult; and in the brain of *Kogia Greyi* the rhinal fissure and the typical (macroscopically only) pyriform lobe are retained in a form as clearly defined as that of any macrosmatic mammal (fig. 4). Professor Haswell, in describing this brain* emphasises the fact that "the most remarkable feature of

* W. A. Haswell, "On the Brain of Grey's Whale (*Kogia Greyi*)," 'Linnean Society of New South Wales Proc.,' vol. 8, 1883 (publ. 1884), pp. 437—439, pl. XXI.

the [basal] region, and perhaps of the whole brain, is the great depth of the ectorhinal fissure, a feature marking off the present form very strongly from *Delphinus*" (p. 438). Since his illustrations do not properly delineate this interesting conformation, Professor Haswell kindly permitted me to examine his specimen; and Mr. J. P. Hill has made me an excellent photograph (of its ventral surface), roughly reproduced in the accompanying drawing (fig. 4). It shows the complete and quite-typical rhinal fissure and the characteristic pyriform lobe. In its anterior part the rhinal fissure is fully a centimetre deep.

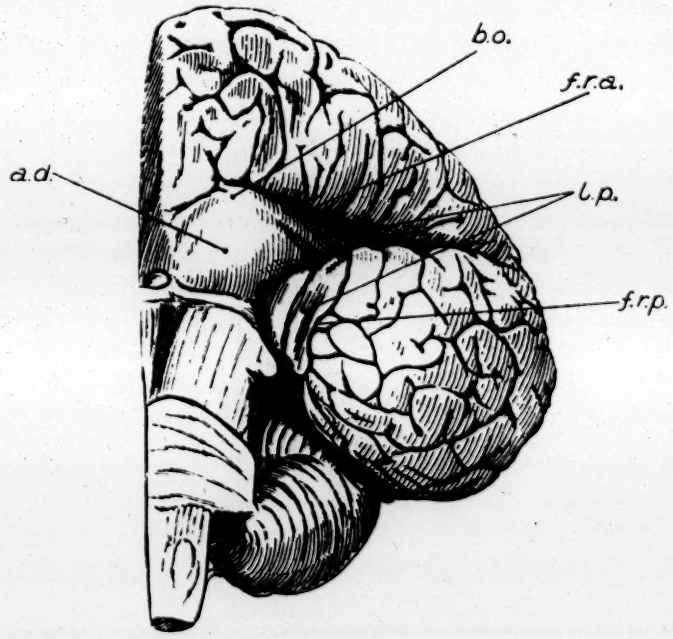


FIG. 4.—Ventral aspect of left hemisphere of *Cogia Greyi*. Reduced approximately one-half. *a.d.*, corpus striatum (desert region); *b.o.*, place occupied by bulbus olfactorius in fœtus; *f.r.a.*, fissura rhinalis anterior; *f.r.p.*, fissura rhinalis posterior; *l.b.*, lobus pyriformis.

The exact reproduction of these characters of the rhinencephalon in an adult anosmatic Cetacean, and the presence of the olfactory bulb in the fœtal Narwhal, show that these toothed Cetaceans were certainly (and probably quite recently) derived from ancestors presenting the normal mammalian type of olfactory apparatus. The absence of the olfactory bulb and peduncle in the Odontoceti cannot, therefore, be considered a just reason for adopting the utterly-improbable suggestion of a nearer affinity of the Archæoceti to the Mystacoceti than to the Odontoceti.

Estimated by the amount of sand which it displaced, the bulk of the natural cast (including that of a considerable quantity of matrix attached to the base of the brain and some small fragments of bone)

is 410 c.c. If the necessary corrections and estimations be made from this gross cubic capacity, the weight of the brain in the Archæoceti must have been considerably less than 400 grammes, and perhaps nearer 300, as against that of the recent Cetacea, which ranges from 455 grammes in *Kogia* (Haswell) to 4,700 grammes in *Balænoptera* (Guldberg).

"The Differential Invariants of a Surface, and their Geometric Significance." By Professor A. R. FORSYTH, Sc.D., F.R.S. Received February 14,—Read March 5, 1903.

(Abstract.)

The present memoir is devoted to the consideration of the differential invariants of a surface; and these are defined as the functions of the fundamental magnitudes of the surface and of quantities connected with curves upon the surface which remain unchanged in value through all changes of the variables of position on it. They belong to the general class of Lie's differential invariants; and some sections of them were obtained about ten years ago by Professor Zorawski, who, for this purpose, developed a method originally outlined by Lie. Earlier, they had formed the subject of investigations by a number of geometers, among whom Beltrami and Darboux should be mentioned.

Professor Zorawski's method is used in this memoir. In applying it, a considerable simplification proves to be possible; for it appears that, at a certain stage in the solution of the partial differential equations characteristic of the invariance, the equations which then remain unsolved can be transformed so that they become the partial differential equations of the system of concomitants of a set of simultaneous binary forms. The known results of the latter theory can then be used to complete the solution.

The memoir consists of two parts. In the first part, the algebraic expressions of the invariants up to a certain order are explicitly obtained; in the second, their geometric significance is investigated.

An invariant, which involves the fundamental quantities of a surface E, F, G, L, M, N (these determine the surface save as to position and orientation in space) and their derivatives up to order n, as well as the derivatives of functions ϕ, ψ, of position on the surface up to order $n+1$, may itself be said to be of order n. The invariants up to the second order inclusive are obtained. It appears that, if two functions ϕ and ψ occur, all the invariants that occur up to the second order can be expressed algebraically in terms of

29 algebraically independent invariants; while, if only a single function ϕ occurs, all the invariants that occur up to the second order can be expressed in terms of 20 algebraically independent invariants.

The significance of these respective aggregates of 29 and of 20 invariants is obtained in connection with curves

$$\phi = 0, \psi = 0,$$

drawn upon the surface. The investigation reveals new relations among the intrinsic geometric properties of a curve upon a surface. In particular, up to the second order, four such relations exist for a single curve; and their explicit expressions have been constructed.

"The Electrical Conductivity of Solutions at the Freezing-point of Water." By W. C. D. WHETHAM, F.R.S., Fellow of Trinity College, Cambridge. Received February 14,—Read March 5, 1903.

The following paper contains an account of experiments which bring to greater concentrations the series of measurements on the conductivities of dilute solutions at the freezing point, communicated to the Royal Society in February, 1900.* The work has been carried on at intervals during the last two years, and was made possible by the kindness of Professor Ewing, who placed a room at the Cambridge Engineering Laboratory at the disposal of the writer and his wife.

The earlier experiments were originally planned in connection with the observations undertaken by Dr. E. H. Griffiths on the freezing points of corresponding solutions; they were therefore conducted in a platinum cell of design similar to that used by Griffiths, with the object of eliminating any solvent action of glass. Any such action would be quite inappreciable at the concentrations used in the experiments now to be described; resistance cells of glass were consequently used, and the labour of observation was much reduced.

The structure of the cells is shown in figs. 1 and 2; that illustrated in fig. 1 was made of Jena glass, and reserved for the more dilute solutions. Each cell is so arranged that by applying a slight exhaustion, the whole of the contents can be drawn up into a bulb; by this means, when water or stock solution is added, complete mixture can be easily secured. The method of preparing the solutions was usually the same as that formerly employed; the pure solvent was placed in the cell, and its weight and resistance determined; weighed quantities

* 'Phil. Trans.,' A, vol. 194 (1900), p. 321.

of stock solution were then added successively, the resistance being measured after each addition. When, however, the solutions approached saturation, this procedure was reversed; the stock solution was first examined, and was then diluted by a weighed quantity of water.

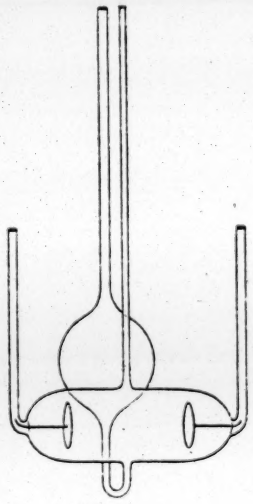


FIG. 1.
($\frac{1}{3}$ full size.)

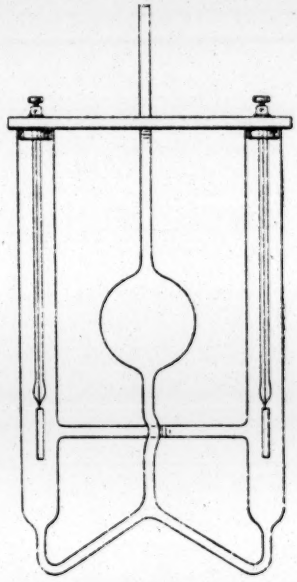


FIG. 2.
($\frac{1}{4}$ full size.)

The resistance cell was placed in a tin bath filled with melting ice. At first, a thermometer was inserted in the cell, and the observations postponed till its readings became steady at 0° centigrade; but it was soon found that the resistance itself gave a more sensitive and convenient means of thermometry. Measurements were taken at intervals till they became constant; the cell being always completely immersed in ice, the result thus obtained shows the resistance at 0°. As an example, the following numbers may be given:—

Time.	Resistance.
12.45	2179
12.55	2191
1.12	2202
1.16	2202
1.18	2202

It was intended to make up the solutions in the resistance cell while it was immersed in the ice, but condensation of water from the atmosphere was found to occur on the inside of the cold vessel when stock solution was added. Before each addition, therefore, the cell and its contents were raised to some temperature near that of the

room by standing the cell for a few minutes in slightly warmed water.

The water used as solvent was first boiled, then distilled in glass from alkaline permanganate, and finally redistilled in a platinum still with a trace of acid potassium sulphate. It was kept in a large stoppered flask of Jena glass till required for the experiments.

The samples of salt used to make the stock solutions of potassium chloride and copper sulphate were the same as those prepared for the earlier experiments; details will be found in the account of those experiments to which reference has already been made. In the cases of barium chloride, potassium bichromate, and magnesium sulphate, the best salt, sold as chemically pure, was obtained, the two salts first mentioned being recrystallised before use. Any probable impurities would not affect the results to an amount equal to the other errors of experiment.

The measurement of the electrical resistance was performed exactly as in the earlier set of experiments. The current from one or two dry cells was alternated by means of a revolving commutator, which was driven by a hand wheel and cord, the connections of a D'Arsonval galvanometer being simultaneously alternated by the same instrument. The alternating currents were passed through a Wheatstone's bridge, in one of the arms of which was inserted the electrolytic cell. This apparatus worked with perfect success; its sensitiveness was enough to enable readings to be taken to an accuracy varying from one part in one thousand to one part in thirty thousand, according to the resistance in the circuit.

The results which were obtained are collected below. In the table headed Potassium Chloride I, and in the tables for all the other salts, the concentrations, given in the first column under m, are calculated in terms of the number of gram-equivalents of salt per thousand grams of solution, while, for the sake of comparison, under Potassium Chloride II, the conductivities are reckoned per thousand grams of water. In the second column under $m^{\frac{1}{3}}$, for convenience in plotting curves, are tabulated the cube roots of the concentrations; the third column, R, shows the measured resistances of the solutions; next comes $k/m = \mu$, the equivalent conductivities; and finally, in the last column, are placed the ratios of μ to its value at infinite dilution, which was estimated from the former set of experiments. It was thought advisable, in view of the freezing point observations now being conducted in Dr. Griffiths' apparatus by Mr. T. G. Bedford, to include magnesium sulphate in the investigation. This substance was not examined in the platinum cell, so that it was necessary to extend the experiments to very dilute solutions. This was done in cell No. 1, which is made of Jena glass, and therefore is not likely to react appreciably with a solution of the nature of magnesium sulphate.

Potassium Chloride I. KCl = 74·59.

In Cell No. 2, the water used had a resistance of $8·3 \times 10^6$ ohms at 0°.

m.	$m^{\frac{1}{2}}$.	R.	$k/m = \mu$.	μ/μ_∞.
0·02393	0·288	7894	755	0·936
0·04261	0·349	4504	744	0·922
0·07924	0·430	2475	728	0·902
0·1459	0·526	1374	712	0·883
0·2441	0·625	835·4	701	0·868
0·4268	0·753	484·3	691	0·857
0·6379	0·861	325·5	688	0·853
0·8924	0·963	232·3	689	0·854
1·0759	1·025	191·9	692	0·857

Potassium Chloride II. KCl = 74·59.

m.	$m^{\frac{1}{2}}$.	R.	$k/m = \mu$.	μ/μ_∞.
0·02394	0·288	7894	754	0·934
0·04275	0·350	4504	741	0·919
0·07971	0·430	2475	724	0·897
0·1475	0·528	1374	705	0·873
0·2486	0·629	835·4	688	0·852
0·4408	0·761	484·3	669	0·829
0·6697	0·875	325·5	655	0·812
0·9575	0·986	232·3	642	0·796
1·1697	1·054	191·9	636	0·789

Barium Chloride. $\frac{1}{2}BaCl_2$ = 104·1.

In Cell No. 2, the water used had a resistance of $8·5 \times 10^6$ ohms.

m.	$m^{\frac{1}{2}}$.	R.	$k/m = \mu$.	μ/μ_∞.
0·02040	0·273	10856	644	0·863
0·05112	0·371	4584	609	0·816
0·1137	0·485	2178	577	0·773
0·2377	0·619	1095	549	0·735
0·4705	0·778	580·4	523	0·700
0·8710	0·955	325·5	504	0·674
1·741	1·203	172·2	476	0·638

Copper Sulphate. $\frac{1}{2}CuSO_4.5H_2O$ = 124·87.

Cell No. 2. The water had a resistance of $6·6 \times 10^6$ ohms.

m.	$m^{\frac{1}{2}}$.	R.	$k/m = \mu$.	μ/μ_∞.
0·02174	0·279	17173	382	0·548
0·07607	0·423	6397	293	0·421
0·2681	0·645	2359	226	0·324
0·5608	0·825	1313	193·9	0·279
0·8440	0·945	951·5	161·1	0·231
1·954	1·250	535·7	136·4	0·196

Potassium Bichromate. $\frac{1}{2}K_2Cr_2O_7 = 147{\cdot}3$.

In Cell No. 1, the water used had a resistance of $7{\cdot}26 \times 10^5$ ohms.

m.	$m^{\frac{1}{2}}$.	R.	$k/m = \mu$.	μ/μ_∞.
0·002278	0·132	3004	710	0·865
0·006047	0·182	1138	706	0·860
0·01475	0·245	472·2	698	0·850
0·04019	0·343	5240*	678	0·826
0·1010	0·466	2202*	642	0·782
0·2577	0·636	917·1*	604	0·736

Magnesium Sulphate. $\frac{1}{2}MgSO_4.7H_2O = 123{\cdot}26$.

In Cell No. 1, the water used had a resistance of $9{\cdot}75 \times 10^5$ ohms.

m.	$m^{\frac{1}{2}}$.	R.	$k/m = \mu$.	μ/μ_∞.
0·00000644	0·0186	511000	702	0·987
0·00001398	0·0241	330000	697	0·980
0·00003218	0·0318	179200	687	0·965
0·00007707	0·0426	84710	680	0·955
0·0003438	0·0701	21230	652	0·916
0·001423	0·113	5554	598	0·840
0·005030	0·171	1881	513	0·721
0·02678	0·299	456·8	397	0·558

Magnesium Sulphate. $\frac{1}{2}MgSO_4.7H_2O = 123{\cdot}26$.

In Cell No. 2, the water used had a resistance of $8{\cdot}7 \times 10^6$ ohms.

m.	$m^{\frac{1}{2}}$.	R.	$k/m = \mu$.	μ/μ_∞.
0·04995	0·368	8067	354	0·498
0·1158	0·487	4083	302	0·425
0·2136	0·598	2494	268	0·377
0·3574	0·710	1654	241·5	0·340
0·6413	0·862	1052·5	211·6	0·298
1·189	1·060	682·2	176·1	0·248
1·812	1·220	540·8	145·7	0·205
2·642	1·383	488·4	110·7	0·1556
3·463	1·513	524·1	78·6	0·1105

In order to obtain the most probable results for the ratio of the equivalent conductivities to their values at infinite dilution, curves were drawn on squared paper between $m^{\frac{1}{2}}$ and k/m, and the smoothed readings taken at the required places. It is usual to call this ratio the coefficient of ionization, but at the high concentrations here dealt

* In Cell No. 2.

with, we cannot assume that it really gives the fraction of the number of the molecules which are at any moment ionized; in the light of probable changes in the ionic fluidity of the liquids, and of the possible existence of complex ions, such an assumption is clearly unjustified. For the sake of convenience, the results previously obtained, as well as those of the experiments now described, are collected in the following table :—

Equivalent Conductivities at 0° referred to the Limiting Value as Unity.

m = Number of Gram-equivalents of Solute per thousand grams of Solution.

m.	$m^{\frac{1}{2}}$.	KCl.	$\frac{1}{2}BaCl_2$.	$\frac{1}{2}K_2Cr_2O_7$.	$\frac{1}{2}CuSO_4$.	$\frac{1}{2}MgSO_4$.
0·00001	0·0215	1·000	1·000	0·991	0·998	0·983
0·00002	0·0272	1·000	1·000	0·980	0·993	0·976
0·00005	0·0368	1·000	0·998	0·952	0·981	0·963
0·0001	0·0464	0·999	0·995	0·929	0·967	0·950
0·0002	0·0585	0·998	9·990	0·902	0·947	0·932
0.0005	0·0794	0·996	0·980	0·880	0·908	0·899
0·001	0·100	0·992	0·969	0·870	0·863	0·864
0·002	0·126	0·987	0·953	0·864	0·807	0·814
0·005	0·171	0·976	0·925	0·863	0·717	0·720
0·01	0·215	0·962	0·896	0·858	0·638	0·659
0·02	0·271	0·944	0·864	0·847	0·557	0·587
0·03	0·311	0·932	0·843	0·834	0·512	0·545
0·05	0·368	0·917	0·813	0·815	0·468	0·497
0·10	0·464	0·896	0·778	0·783	0·405	0·435
0·20	0·585	0·874	0·742	0·749	0·348	0·380
0·40	0·737	0·858	0·710	—	0·294	0·322
0·50	0·794	0·855	0·699	—	0·275	0·313
1·0	1·000	0·856	0·665	—	2·230	0·264
1·2	1·063	0·860	0·657	—	0·218	0·248
1·5	1·145	—	0·645	—	0·208	0·227
2·0	1·260	—	0·632	—	0·194	0·192
3·0	1·442	—	—	—	—	0·133

In the earlier set of experiments, approximate values only were obtained for the absolute equivalent conductivities, changes in the adjustment of the platinum cell between the experiments on each salt causing a slight uncertainty in the cell constant. From the values of the constants of the glass cells now used, it is possible to calculate throughout the whole range of concentration of both sets of observations the exact equivalent conductivities of the salts investigated. The results are given below, and are expressed in Kohlrausch's units, in which the electrical conductivities, measured in reciprocal ohms, are divided by the concentrations of the solutions, measured in gram-equivalents per cubic centimetre.

Equivalent Conductivities at 0° in Absolute Units.

m = number of Gram-equivalents of Solute per thousand grams of Solution.

m.	$m^{\frac{1}{2}}$.	KCl.	$\frac{1}{2}BaCl_2$.	$\frac{1}{2}K_2Cr_3O_7$.	$\frac{1}{2}CuSO_4$.	$\frac{1}{2}MgSO_4$.
0·00001	0·0215	807	746	813	696	699
0·00002	0·0272	807	746	804	692	694
0·00005	0·0368	807	745	781	684	685
0·0001	0·0464	806	742	763	674	676
0·0002	0·0585	806	739	740	660	663
0·0005	0·0794	803	731	722	633	639
0·001	0·100	800	723	714	602	614
0·002	0·126	796	711	709	563	579
0·005	0·171	787	690	708	500	512
0·01	0·215	776	669	704	445	468
0·02	0·271	761	645	695	388	417
0·03	0·311	752	629	685	357	387
0·05	0·368	740	607	669	326	353
0·10	0·464	723	581	643	282	309
0·20	0·585	705	554	615	243	270
0·40	0·737	692	530	—	205	229
0·50	0·794	690	522	—	192	222
1·0	1·000	690	496	—	160	188
1·2	1·063	694	490	—	152	176
1·5	1·145	—	481	—	145	161
2·0	1·260	—	472	—	135	136
3·0	1·442	—	—	—	—	94·5

"The Resistance of the Ions and the Mechanical Friction of the Solvent." By FRIEDR. KOHLRAUSCH, Foreign Member R.S. Received February 17,—Read March 5, 1903.

(Translated into English for Dr. Kohlrausch by Dr. L. Austin.)

Messrs. Bousfield and Lowry in their interesting paper, "The Influence of Temperature on the Conductivity of Electrolytic Solutions,"* have discussed a hypothesis recently advanced by me. In this I stated the probability that the conductivities of all aqueous solutions approach, with decreasing temperature, a zero value at about the same temperature, and that the cause of this phenomenon is to be looked for in the disappearance of the fluidity of water. This hypothesis was very briefly mentioned, as it were, in parenthesis, in the midst of the discussion of the numerical data which formed the main portion of the paper.†

* Bousfield and Lowry, 'Roy. Soc. Proc.,' vol. 70, p. 42, 1902.

† Kohlrausch, "Uber den Temperatureinfluss auf das elektr. Leitvermögen von Lösungen, insbesondere auf die Beweglichkeit der einzelnen Ionen im Wasser," 'Sitz. Ber. Berlin Akad.,' 1901, p. 1028.

I beg the honour of laying before the Royal Society the following more complete consideration of the subject.

The above-mentioned article had for its object the study, from careful measurements made at my request by Mr. Déguisne,* of the influence of temperature on completely dissociated, that is, infinitely dilute aqueous solutions of strong electrolytes, and the deduction from this of the temperature coefficients of the single ions.

Mr. Déguisne expresses the influence of temperature on the conductivity κ, starting from 18° as a mean temperature, in the form of the quadratic interpolations equation

$$\kappa_t = \kappa_{18}[1 + \alpha(t-18) + \beta(t-18)^2]$$

and shows that this closely represents his observations between 2° and 34° C. I shall confine myself to the consideration of this formula.

(1.) *Numerical values.*—In the table, under Λ is given the equivalent conductivity of infinitely dilute solutions,† then the coefficients α and β for 1/1000 and, for the neutral salts, observed by Déguisne, α also for

	Λ.	α for		β.
		1/10000.	1/1000.	
KCl	130·1	0·0216	0·0217	+0·000066
KF	111·3	..	226	069
KNO_3	126·4	210	211	062
K_2SO_4	133·4	223	223	077
KOH	239	..	190	033
NH_4Cl	130	219	219	068
NaCl	109·0	228	227	084
NaF	90·2	..	240	100
$NaNO_3$	105·3	220	221	075
Na_2SO_4	112·2	234	234	097
$NaC_2H_3O_2$	78·5	..	242	110
$NaC_3H_9O_2$	69·2	..	244	111
LiCl	98·9	..	233	091
$BaCl_2$	121·7	227	226	083
$Ba(NO_3)_2$	118·1	221	220	076
$AgNO_3$	115·8	216	216	+0·000067
HCl	383	..	165	−0·000015
HNO_3	380	..	163	−0·000017

1/10000 normal solutions, corrected by me for the impurity of the water. Mr. Déguisne has kindly furnished me with the data regarding the water in each of his solutions. After applying these corrections the values for 1/10000 and 1/1000 normal do not continue to show the systematic changes, which from the observations appeared to

* Déguisne, "Temperatur-Koefficienten des Leitvermögens sehr verdünnter wässriger Lösungen," 'Dissertation, Strassburg,' 1893.

† Kohlrausch u. v. Steinwehr, 'Sitz. Ber. Berlin, Akad.,' 1902, p. 581.

make the reduction to completely dissociated solutions very uncertain, but now show only differences of irregular sign within the limits of the errors of observation.

From this we may assume that the true values of the coefficients for the concentration 1/10000 would be practically the same as those for complete dissociation. But, as a matter of fact, in the case of such dilute solutions considerable uncertainty underlies the observations themselves, on account of the variations of the solutions with time, as well as the corrections for the conductivity of the water, on account of its magnitude and somewhat uncertain theoretical basis. These facts are especially true in the case of the acids. Therefore, the values found for the concentration 1/1000, in which these two sources of error need scarcely be considered, must be looked upon as experimentally much more accurate. Then too, the condition of ionization of very dilute solutions of weak salts is always somewhat uncertain on account of possible hydrolytic action. In order to treat all electrolytes alike, I shall therefore choose the coefficients α and β of the concentration 1/1000.

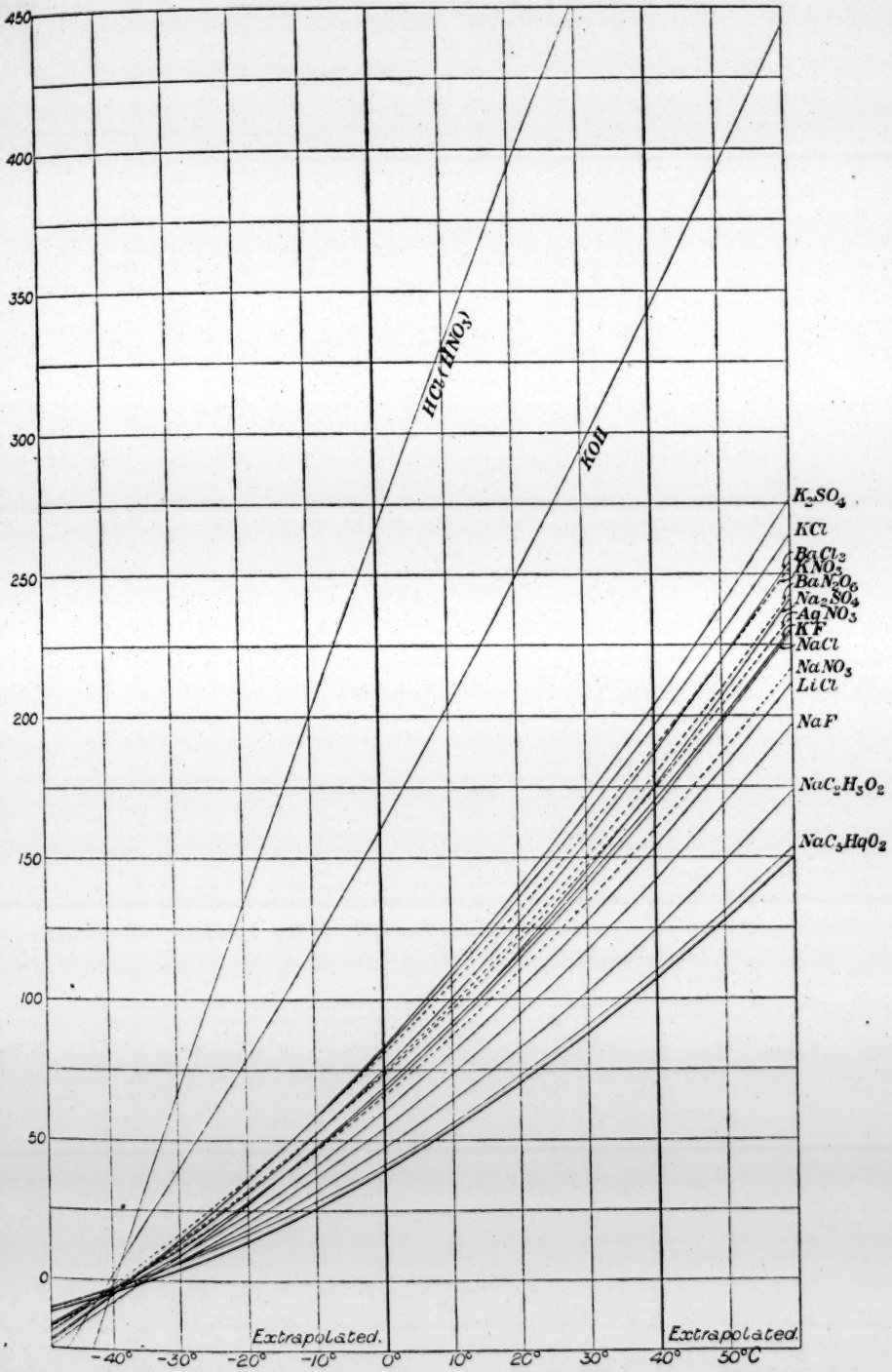
The errors which may arise from the fact that these solutions lack from 2 to 5 per cent. of complete dissociation cannot at present be avoided. Since the values of the constants themselves differ but little from those for a concentration of 1/10000, we may assume that these errors are not great. The material is not yet complete enough to allow the application of the corrections proposed by Mr. Whetham. The values may indeed be capable of improvement, but I am of the opinion that the following conclusions drawn from them are in the main correct.

In order to fill certain gaps in the series of the coefficients, I have added to the electrolytes investigated by Déguisne my own observations on 1/1000 normal solutions of potassium and sodium fluoride, sodium acetate and lithium chloride. The coefficients here are derived only for the temperatures 10°, 18°, and 26°, and will therefore be less accurate than in Déguisne's results.

(2.) *Values of the Conductivity at Different Temperatures.*—My conclusion, that by extrapolating the quadratic formula below 0°, the conductivities of all electrolytes approach a zero value within a narrow range of temperature, rested upon an empirically established connection between the coefficients α and β. I will introduce here, however, the more easily followed graphic representation (fig. 1) plotted from the single coefficients. The extrapolated portions of all sixteen curves converge toward the same region and pass through zero between $-35°$ and $-41°$.*

* NH_4Cl would nearly coincide with KCl; $MgSO_4$ on account of the large changes in its coefficient with increasing dilution is uncertain and therefore omitted; it would probably lie near Na_2SO_4.

FIG. 1.



In regard to the lowest curve, *cf.* 4. The fact that the greatest differences exist for KOH and KF may be connected with the smaller accuracy of these curves. But on this we will lay no stress.

(3.) *Discussion of Results.*—We may derive from the above fact the certainty that the individual differences of the electrolytes come under a common law, the degree of accuracy of which must, however, remain unsettled. This law I have stated as follows: for dissociated aqueous solutions the coefficient β of the quadratic member can be approximately expressed in terms of the coefficient α of the linear member, in the form $\beta = C(\alpha - A)$, where C and A are constants common to all electrolytes. One sees at once that this law is identical with the other; all curves of the expression $(1 + \alpha t + \beta t^2)$ pass through the same point, having for its abscissa $-1/C$.*

The proposed constants have the following values, taking 18° C. as the point from which the temperature is reckoned, C = 0·0163, A = 0·0174. The convergence takes place at the point where $t - 18 = -1/0{\cdot}0163 = -61$, or $t = -43°$. On account of the small difference between C and A, this point lies not far from the zero axis. If C and A were identical, the extrapolation according to the quadratic formula would show that the conductivity of all electrolytes becomes zero at the same temperature. Introducing this critical temperature t_0, all electrolytes could be nearly represented by a formula containing but two individual constants,

$$\kappa_t = P(t - t_0) + Q(t - t_0)^2.$$

On the one hand, I consider it impossible that the inequality of A and C, and the resulting deviations from a common point of convergence on the zero axis are produced by errors of observation. Even the circumstance that we have no completely dissociated solutions can scarcely have so great an influence. On the other hand, it appears very improbable that the approximate equality of the constants A and C is purely accidental. The deduction that the extrapolated curves all have a nearly common point of convergence appears to me especially worthy of notice in that this point lies approximately at the zero value of the conductivity. The importance of this is still more increased by the fact that if the mobility of the water particles be extrapolated according to the same formula, it becomes zero at about the same temperature (*cf.* 4).

(4.) *Variations of the Fluidity of Water with Temperature.*—The relations which have just been mentioned concerning the motions of the ions in water assume a greater interest when they are compared with the mobility of the water particles themselves. The fluidity (the reciprocal of the viscosity) of water when calculated in the same way as the conductivity, with the quadratic formula, is represented by the lowest curve.

That the best observations on the fluidity of water agree excellently

* The coincidence in the drawing differs a little from this, because each of the expressions is multiplied by its corresponding A.

at moderate temperatures with the quadratic interpolation formula, already applied by Poiseuille, has long been known to me. The literary priority regarding this observation belongs, however, to Messrs. Bousfield and Lowry.

In my calculations I have made use of the values chosen by Mr. Heydweiller,* as the most probable between the limits of 0° and 30°. These values were selected from different observers, especially Messrs. Thorpe and Rodger. Since the constants were calculated for this range of temperature, they have practically the same signification for the fluidity of water as the former constants for the conductivity.

The formula

$$\phi_t = 55{\cdot}68 + 1{\cdot}981t + 0{\cdot}0105t^2 \text{ C.G.S.}$$

or

$$\phi_t = 94{\cdot}74[1 + 0{\cdot}0249(t - 18) + 0{\cdot}000111(t - 18)^2] \text{ C.G.S.}$$

represents the fluidity ϕ within the limits of the table, 0° to 30°, with a maximum error of 1/1000, that is, with about the same degree of accuracy as Déguisne's formula for the conductivity. As far as 90° the error would not exceed 1/100.

The curve represented in the figure instead of having the factor 94·74 (which has no reference to the conductivity), was given the arbitrary factor 67·0 in order to give the curve the desired position close to the lowest curve of conductivity, that of sodium valerate.

The coincidence of the two curves is striking, indeed the coefficients 0·0249 and 0·000111 differ little from those of sodium valerate 0·0244 and 0·000111.† The curves of conductivity are cut by the fluidity curve approximately in the same region to which they converge. The curve of fluidity passes through zero at − 34° C.‡

Messrs. Bousfield and Lowry calculated from the measurements of Thorpe and Rodger the coefficients 0·0251 and 0·000115. This curve differs very little from mine, especially in the neighbourhood of the crossing point.

* Mr. Heydweiller calculated at my request the table for the 'Lehrbuch der Praktischen Physik' (Tab. 20a, 1901).

† The same would apply for Déguisne's observations on Na_2HPO_4 (0·0241 and 0·000105) and on $NaHC_4H_4O_4$ (0·0241 and 0·000109), which, however, on account of the unknown constitution of these salts in solution, I have left out of account.

Messrs. Bousfield and Lowry further called attention to the fact that the temperature change which I have found for ordinary distilled water (practically a very dilute solution of CO_2) corresponds with the temperature change of fluidity.

‡ It is also to be mentioned that Messrs. Lyle and Hosking, from their interesting observations on the viscosity and the electrical resistance of 0·1 to 4 normal solutions of NaCl between 0° and 100° draw the conclusion: "The curves so arrived at are remarkable, in that they indicate that for solutions of the strengths experimented with, both the fluidity and the sp. mol. conductivity vanish at a temperature of −35°·5 C." The manner of extrapolation is not dealt with.—'Phil. Mag.,' May, 1902, p. 496.

The fact is therefore established that the temperature change of the fluidity of water is nearly the same as that of the conductivity of dissociated aqueous solutions of electrolytes which have a large temperature coefficient. Even if nothing more was known than this fact, the question of a connection between the electrolytic and the mechanical motion in water must be considered a matter for serious discussion.

(5.) *Discussion of the Extrapolation.*—Extrapolation of an empirical formula over a wide range can never be considered as necessarily representing the truth. This is especially true in a case like the present, where at low temperatures it is applied to a state of matter other than that for which the formula was originally deduced. It is *à priori* impossible for the formula to hold where its extrapolation gives to the conductivity or the fluidity a value zero. Since these quantities are from their very nature positive, negative values are physically impossible. Therefore the cutting of the zero axis by the curve at an acute angle is *à priori* inadmissible, just as, for example, the assumption is inadmissible that the Joule heating effect is proportional to the current strength, or that the kinetic energy is proportional to the velocity. A quantity from its nature positive can, as it becomes zero, have a finite differential quotient as function of another quantity, only when the other quantity cannot vary beyond the critical point. This can be considered identical with the impossibility of negative values. In reality the conductivity and the fluidity must reach the zero value in a curve which is tangent to the axis of temperature. (Becoming zero through a discontinuous process as in freezing is, of course, something entirely different.)

Therefore the quadratic formula, in spite of the fact that it shows such a remarkably wide range of applicability, must be replaced by another expression before the zero value is reached.

The above explanation shows that my view of the "critical temperature" of the fluidity and the conductivity of water as derived from the quadratic formula, does not materially differ from that of Messrs. Bousfield and Lowry. This temperature is only a quantity by which one constant of the ordinary formula can be replaced; but the importance of the constant now introduced is verified, in that now the individualities of the ions, if they do not entirely vanish, at any rate disappear except for small differences. Further, the remarkable fact follows, that approximately the same constant may be introduced in the temperature formula of the viscosity of water. This number, entering as a temperature, may therefore be called a fundamental constant of water, of course with the reservation which follows from the fact that it varies by several degrees in the different cases.* (*Cf.* 3.)

* The objection that the use of such a constant may be responsible for the

(6.) *Other Formulæ.*—Among the former attempts to derive an empirical formula from the behaviour in ordinary temperatures, which does not *à priori* lose its significance as the conductivity or the fluidity approaches zero, that of Slotte, $\phi = \phi_0 (1 + bt)^n$, must be given the first place. This has recently assumed a greater importance on account of the extensive work of Thorpe and Rodger. Here, in the case of the fluidity, the condition that $d\phi/dt = 0$ where $\phi = 0$ is fulfilled, for here everywhere $n > 1$. For the conductivity, however, nothing is gained, since in the case of the acids $n < 1$, and there $d\kappa/dt = \infty$ for $\kappa = 0$; there is, therefore, no object in recalculating our results according to this formula. However, in a former paper I have given the preference to Slotte's formula in a remark concerning fluidity. To try to bring the coefficients of this formula into a relationship with those of the quadratic formula was indeed not allowable.

(7.) *Experimental Indications.*—The attempt to draw conclusions in regard to the region near the zero value from the course of the phenomenon in the region where the fluidity &c. have values of considerable magnitude, would have, of course, very little prospect of success. But perhaps the attempt to follow the fluidity of water or the conductivity of dilute solutions down to lower temperatures would be more successful than we think, if made in closed vessels of small dimensions.

Up to the present time we are acquainted only with phenomena from which uncertain conclusions from analogy can be drawn. The idea may be pretty certainly held as probable, that the viscosity and the electrical resistance finally increase more slowly than the quadratic formula extrapolated would indicate. In regard to viscosity, I remember the investigations of Tammann on the freezing of overcooled liquids, and Ostwald's observations on salol. The phenomenon of the gradual solidification of alcohol at low temperatures also leads to the same conclusion.

Numerous observations of the same kind in regard to electrical behaviour are also recorded. The very slow increase of the conductivity of glass with increasing temperature is well known. A quantitative determination of this has been made by Messrs. Bousfield and Lowry. But how far a heterogeneous mixture like glass can be considered parallel to a dilute ionised solution is doubtful. In the same way, experiments such as those of Lehmann on the electrolysis

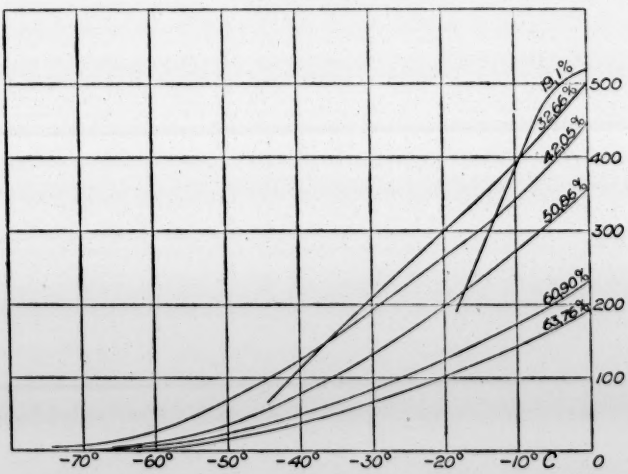
introduction of errors is without much weight; it is, at any rate, practically the same case as that of the gas formula. This is written, making use of the absolute zero, $vp = \text{const.}\ (t_0 + t)$, where $t_0 = 273$, although it is certain that the result $v = 0$ for $t = -t_0$ is false, so that the formula must assume another form before v approaches its zero value. The constant of the gas formula loses its significance at the point where the gas passes into the liquid state. The same is true for the temperature constant of the fluidity, which we have introduced, when it passes out of the liquid state.

of solid silver iodide, or those of W. Kohlrausch on its conductivity at different temperatures, scarcely form a basis for conclusions in regard to solutions in water, particularly as the behaviour of silver chloride and silver bromide is quite different from that of silver iodide.

In addition, it is difficult to compare substances which are so highly concentrated, in comparison with ordinary electrolytes, with dilute solutions. All these conductivities reduced in the ratio of the concentration, as, for example, the measurements made on hot glass of large surface and small thickness by means of the potentiometer, would probably have to be considered zero, that is, smaller than the errors of observation, in comparison with our dilute salt solutions at moderate temperatures.

Of the greatest interest for our problem are the recently published measurements of electral conductivity as far as $-70°$ C., which Mr. Kunz* was led to make through my remarks on the relation between conductivity and temperature. He was unfortunately unable (as was I also) to sufficiently over-cool dilute solutions, and the measurements were therefore made on strong solutions of sulphuric acid of at least 4 gr. equiv./litre. Notwithstanding the fact that no certain conclusions can be drawn in regard to dilute solutions, Mr. Kunz's values are of sufficient interest to represent graphically. The curves are marked with the percentage concentration of the solutions (fig. 2).

Fig. 2.



The conductivity of the strongest solutions sinks gradually with the temperature, and reaches at $-70°$ a relatively small value. It is impossible to follow the more dilute solutions to so low a temperature on account of their freezing. But it is evident from the observations that the more dilute they are, the more rapid is the rate of change of the

* 'Comptes Rendus,' vol. 135, 1902, p. 788.

conductivity, so that it may be supposed that the 19 per cent. solution would reach a relatively negligible value at a much higher temperature than the stronger solutions, if it were possible to follow it.

Mr. Kunz adopts the view that the electrical resistance is due to friction. It is his opinion, indeed, that the conductivity would disappear only at the absolute zero of temperature; this conclusion can hardly be supported by his observations, as his lowest temperatures are still $+200^{\circ}$ absolute.

(8.) *The Temperature Coefficients of the Single Ions.*—For these* I have recently published values. It is of importance to us that the temperature coefficients of univalent monatomic ions appear to be functions of mobility, decreasing as the mobility increases. Complex and multivalent ions as groups deviate from this series, so that in this relationship we have a new criterion for univalent elemental ions. The largest temperature coefficients of the ions approach that of water.

(9.) *The Electrolytic Resistance Considered as Friction of the Solvent.*—In the common view concerning the motion of the ions, an assumption is tacitly made which in other cases we do not consider justifiable. In the relative motions of adjacent particles we assume a discontinuity only in the case of friction between two rigid bodies where this by definition must occur. Even here it is impossible to deny that on the actual surfaces of contact there may be connected with the motion a rubbing away of particles which produces a continuous variation of velocity from one to the other.

When a fluid is in question, whether in contact with another fluid or with a solid, we concede no finite variation of velocity in two points at an infinitesimal distance from each other. The primitive assumption, until recently held to be correct, in the case of mercury on glass, that the fluid in actual contact with the solid moved with a finite velocity, would demand that the external friction be infinitesimal in comparison with the internal. This is now, to the best of my knowledge, entirely given up.† The idea of discontinuity, however, we employ in regard to the ions when we think of them as moving through the solvent without connection with it.

In addition to the objection of discontinuity there exist also the following difficulties in this assumption. In the first place, it is difficult to see how the electrical energy passes into the solvent in the form of heat, unless the latter takes part in the motion of translation. Further, it seems probable, from the fact of the ionisation of the salts,

* 'Sitz. Ber. d. Berlin. Akademie,' 1902, p. 572. The values here given are strengthened by the fact that a linear connection between α and β (*cf.* 3) appears *à priori* in the case of the single ions as well as in the case of electrolytes.

† Comp. Warburg, 'Pogg. Ann.,' vol. 140, 1870, p. 379. The "slipping" of rarefied gases on solid surfaces, established by Kundt and Warburg, being a separate phenomenon, need not be considered here.

that forces must exist between the ions and the water. Ciamician,* twelve years ago, concluded that this must lead to the hypothesis of a water-shell about the ion. The attempt was also made to measure this hydration, by using a method proposed by Nernst.

We may, therefore, look upon it as probable that the moving ion carries with it a mass of adhering solvent, just as a moving immersed solid carries with it a portion of the liquid, and we will endeavour on this basis to construct a new representation of electrolytic resistance. According to this the mechanics of electrolysis assume an appearance quite different from the old hypothesis of isolated ions. The resistance to motion takes place not directly between the solvent and the ions, not between H_2O and K or Cl, &c., but it is a phenomenon of friction between the particles of the solvent itself, modified by the fact that the accompanying shell of solvent may be thin enough to allow the ion to act through it, to a certain extent, upon the outer liquid.

It is impossible at first to make anything more than this rough sketch of the hypothesis, and the more so, as the expressions "continuous" and "discontinuous" must be especially defined if we are to apply them to molecular processes. The expression "continuous" stands from its very nature in contradiction to the atomic or molecular hypothesis; and in the case of solutions, and especially in electrolysis, a molecular representation seems to be the only one which is scientifically thinkable.

I hardly need after this to say that in our hypothesis we shall not claim to be able to differentiate strictly between the outer portions of the solution and those parts which have separated themselves from the rest as the accompanying atmosphere of the ion. A continuous change in the condition of motion from the moving ion to the solvent contradicts a strict differentiation. Fundamentally, however, the same is true for all atmospheres, even for that of the earth.

For the sake of brevity, we will retain the expression that an atmosphere of the solvent takes part in the motion of the ion. In the light of this hypothesis, I believe that all the phenomena which have been here described become so much clearer that this fact itself serves as a remarkable experimental support of the hypothesis.

(10.) *Hypotheses and Conclusions.*—The hypotheses are: About every on moves an atmosphere of the solvent, whose dimensions are determined by the individual characteristics of the ion. The atmospheres of multivalent or compound ions differ from those of monatomic ions. Data are at present lacking for a more detailed representation.

The electrolytic resistance of an ion is a frictional resistance that increases with the dimensions of the atmosphere.† The direct action

* Zeitschr. f. physikal. Chem.,' vol 6, 1890, p. 405.

† The resistance of a sphere is proportional to its radius. Kirchhoff, 'Vorles. üb. math. Physik.,' 1, 380, 1897 (herausgegeben von W. Wien).

between the ion and the outer portion of the solvent diminishes as the atmosphere becomes of greater thickness.

Conclusions.—(*a.*) The electrical resistance of an ion, expressed in mechanical units, must be of the same order of magnitude as the mechanical frictional resistance of a molecule of the solvent; a law whose assumption, as I some time ago showed, "leads to an expression for the distance between the molecules which is comparable with the usually accepted views in regard to this quantity."*

(*b.*) The empirically discovered law that the temperature change of the resistance of the most sluggish ions is very like the temperature change of the viscosity of water, becomes now understandable. For ions of large resistance we must assume that the atmosphere is of considerable thickness, and hence the action of the ion itself on the outer portion of the solvent will be small. As a limiting case, for a very sluggish ion there will be only the friction of water against water, and the electrolytic resistance will have the same temperature coefficient as the viscosity of water, provided that the atmosphere itself does not change its dimensions with the temperature. If, however, the atmosphere become, for example, smaller with increasing temperature, the temperature gradient of the conductivity might be greater than that of the fluidity. According to the observations now at hand, this would seem to be the case for the slowest moving univalent ion Li. Even here, however, the differences scarcely exceed the errors of observation.

(*c.*) I now come to the remarkable relationship between the mobility of the ions and their temperature coefficients, which was mentioned in Section 8. This first led me to seek a general explanation for the electrolytic resistance in the idea of a water atmosphere, in order to escape being compelled to explain this otherwise unreconcilable fundamental characteristic of the ions as a *deus ex machina*.

Assuming as the single fundamental characteristic of each univalent monatomic ion the formation of a water atmosphere which varies according to the nature of the ion, the mobility of this complex on the one side, and its temperature coefficient on the other, will be functions of these atmospheric formations, and therefore both quantities must hold functional relations to each other. We know too little of the molecular forces at present to attempt to describe this connection more exactly. But for the case in which the water shell is so thick that the ion exerts no force beyond it, the resistance to motion becomes simply a matter of water friction, which explains the fact that the most sluggish ions have nearly the same temperature coefficients as the viscosity. In the case of smaller aggregations, we must remain content with the fact that we have at least the possibility of a fundamental explanation.

* 'Göttinger Nachrichten,' 1879, p. 1.

There are two experimental questions which are of importance although difficult to answer: first, whether the functional relationship is exact or only approximate, and second, whether the positive and negative ions are fully identical in regard to this relationship.

That the non-elementary ions also show as their greatest temperature coefficients that of water friction, but that they as groups differ from the elementary ions, is to be expected. The latter fact cannot be quantitatively explained. Here also it will be necessary to wait for more exact experimental data to settle the question.

(*d.*) Finally, the indication of the temperature formula that the mobility of all the ions converges towards zero (*cf.* 2 and 3) at about the same temperature, is a logical result, if the electrolytic resistance is in reality a mechanical friction. The fact that the formula for the fluidity of water takes part in this convergence, gives the hypothesis further support.

It does not seem at all impossible that the deviations from a strictly common zero point, found in extrapolating the different formulæ, have a systematic cause. These divergences seem to indicate that the more mobility an ion shows at ordinary temperatures, the more slowly relatively it loses the residue of its mobility as the solvent becomes more viscous. The mobility of the water molecules themselves becomes small at a comparatively high temperature, where such ions as K, Cl, NO_3, SO_4, and even more, OH and H still possess a considerable residue of electrolytic mobility. Such a relationship does not seem at all improbable.

In the foregoing pages I have sought to find a cause for the electrolytic resistance in the single fundamental characteristic of the ions, their hydration, that is, their ability to form atmospheres from the solvent. These views form a hypothetical sketch for the completion of which much is still wanting. It appears to me, however, complete enough to invite one to its experimental or theoretical continuation.

"Upon the Immunising Effects of the Intracellular Contents of the Typhoid Bacillus as obtained by the Disintegration of the Organism at the Temperature of Liquid Air." By ALLAN MACFADYEN, M.D. Communicated by LORD LISTER, O.M., F.R.S. Received February 19,—Read March 12, 1903.

In a previous communication* it was shown that it was possible to disintegrate mechanically the typhoid bacillus at the temperature of liquid air, and to obtain the cell-juices of the organism.

The typhoid cell-juices obtained by this method on inoculation into animals proved toxic or fatal. It was, therefore, concluded that the typhoid bacillus contained within itself an intracellular toxin.

It remained to test the typhoid cell-juices for immuninising and other properties. The preliminary experiments in this direction, which form the subject of the present note, were made upon the monkey. The monkey was selected as an animal most likely to furnish data of possible application to man. For this purpose the typhoid cell-juice was administered subcutaneously to the monkey. The injections did not produce any general symptoms beyond a transient rise in temperature, whilst the material was quickly absorbed after each injection without any traceable local effect. In this manner doses of 0·5 to 1 c.c. of the material were injected at intervals. An immediate result was the agglutination of the typhoid bacillus by the serum of the blood of the treated monkeys, whereas no such effect was produced by the serum of monkeys which had not been treated. This furnished useful evidence that the animals were under the influence of cell-juices derived from the typhoid organism. The injections were repeated at intervals of three to four days, and after a lapse of four to six weeks the animals were bled. The serum obtained was then tested for immunising properties. The test objects were (1) a virulent culture of the typhoid bacillus, and (2) the intracellular toxic juice of the same organism. A varying amount of the virulent bacilli and of their toxic cell-juice was mixed with a varying quantity of the serum. The respective mixtures were then injected into the peritoneal cavity of the guinea-pig.

The broth cultures of the typhoid organism used in the experiments were *per se* lethal in doses of 0·1 c.c. in five to ten hours. The typhoid cell-juices were fatal in doses of 0·2 and 0·1 c.c. in three to five hours, and in doses of 0·05 c.c. in about twelve hours. The serum was thus tested for (1) specific antibacterial and (2) specific antitoxic properties.

The experiments showed that the serum of the monkey, after injections of the typhoid cell-juices, possessed antibacterial and antitoxic

* 'Roy. Soc. Proc.,' *supra*, p. 76.

properties, inasmuch as the serum protected the experimental animals against the bacilli, and also against an intracellular toxin obtained from them.

A simultaneous injection of (1) serum with the bacilli, and (2) serum with the toxic cell-juice produced no lethal or toxic effects. The control animals, on the other hand, invariably succumbed.

It was further investigated whether the serum possessed preventive and curative properties. The serum from treated monkeys was injected into guinea-pigs, one injection being made in each instance, and the same animals received at an interval of 12 to 24 hours lethal doses of the typhoid bacillus and of its toxic intracellular juice respectively. The treated animals survived the test, whilst the control animals succumbed. It was therefore concluded that the serum had protective properties.

A third series of guinea-pigs received lethal doses of the typhoid bacillus and of its toxic cell-juice respectively. The serum was then injected at various intervals into individual animals. It was found that the lives of the animals could be saved by one injection of the serum from a fatal infection or intoxication, even when half of the lethal period had elapsed in each instance. The serum, therefore, possessed curative properties.

In view of the above results it appeared desirable to test the effect of the typhoid cell-juices upon animals larger than the monkey, in order that a greater amount of serum might be obtained conveniently and tested quantitatively as regards antibacterial and antitoxic properties. These experiments are at present in progress, and the results will be communicated in due course.

From the experiments made upon the monkey it would appear:—

(1.) That by the injection of the intracellular juices of the typhoid organism into the monkey it is possible to obtain a serum with both antibacterial and antitoxic properties.

(2.) That such a serum possesses curative and preventive properties as regards the typhoid bacillus and an intracellular toxin present in the same organism.

It is believed that this research has afforded for the first time proof that, in the case of one species of pathogenic bacterium, the intracellular juices of the organism, when injected into a suitable animal, give rise to the production of a serum which is both bactericidal to the organism itself and antitoxic as regards a toxin contained in its substance. How far such properties of the cell-juice are shared by other pathogenic microbes must be the subject of further inquiry.

Experiments have been undertaken, with the aid of the cold-grinding methods referred to, with reference to other cells and organisms at the Jenner Institute of Preventive Medicine, where the above investigation has been conducted.

"On the Histology of *Uredo dispersa*, Erikss., and the 'Mycoplasm' Hypothesis." By H. MARSHALL WARD, Sc.D., F.R.S., Professor of Botany in the University of Cambridge. Received February 13,—Read March 12, 1903.

(Abstract.)

The paper deals with a detailed study of the histological features of the germination, infection, and growth of the mycelium of the Uredo in the tissue of grasses. Primarily, the figures refer especially to the *Uredo* of *Puccinia dispersa* in the tissues of *Bromus secalinus*, but comparisons are made with the behaviour of this and other Uredineæ—*e.g.*, *Puccinia glumarum* and *P. graminis*—in the tissues of other grasses and cereals.

The research, which has been carried on over a year and a half and has involved the preparation and microscopic examination of thousands of sections, is principally based on the application of improved hardening and staining methods to preparations from tube cultures of the grasses concerned, the leaves of which were infected at definite spots. These tube-cultures were prepared according to the method previously described.* At definite intervals after sowing the spores—*e.g.*, after 1, 2, to 6 and 8 days—the infected areas were removed and placed in fixing solutions, and the life-history of the fungus traced step by step, and controlled by reference to uninfected areas.

The full paper is illustrated by numerous figures, and deals with the behaviour of the nuclei, vacuoles, septa, branches, haustoria, and other details of the hyphæ up to the commencement of spore-formation.

The relations of the hyphæ and haustoria to the cell-contents of the host are critically examined, and the cumulative evidence not only fails to support Eriksson's *Mycoplasm* hypothesis; but is completely subversive of it, so far as histological facts are concerned.

Eriksson's hypothesis, which refers the epidemic outbreaks of rust to the sudden transformation into the mycelial form of a supposed infective substance, previously latent and invisible in the cytoplasm of the host, is shown to be untenable because the *corpuscules spéciaux* of this author are proved to be the cut-off haustoria of the fungus.

Eriksson supposes that these *corpuscules* (haustoria) are formed by the hitherto latent germs in the host-cells, growing up in the cells into vesicles, which then pierce the cell-walls and give rise to hyphæ in the intercellular spaces.

The present paper shows that Eriksson has entirely reversed the

* "On Pure Cultures of a Uredine, *Puccinia dispersa* (Erikss.)" 'Roy. Soc. Proc.,' 1902, vol. 69, p. 461.

true order of events. The haustoria have been formed by the hyphæ, and figures are given showing every stage in their development. The first haustorium may be formed by the infecting tube immediately after its penetration through the stoma, and figures are given showing the remains of the germ-tube outside a stoma, the swelling of its tip over the stoma into an appressorium, the passage through the stomatal cavity, and its development into a vesicular swelling whence the true infection tube arises, which latter may at once put forth a haustorium. In some cases all these latter phenomena are visible in one and the same preparation.

The author expresses his thanks to Miss E. Dale, of Girton College, for valuable aid during the later stages of the work, in the embedding and cutting of numerous sections.

"The Œstrous Cycle and the Formation of the Corpus Luteum in the Sheep." By FRANCIS H. A. MARSHALL, B.A. Communicated by Professor J. C. EWART, F.R.S. Received February 17,—Read March 12, 1903.

(Abstract.)

Introduction.—A preliminary account of this investigation was communicated to the Royal Society in 1901, and published in the PROCEEDINGS for that year. Subsequently the work has been carried further, and recently brought to a conclusion.

The Œstrous Cycle.—In Scotch black-faced sheep the length of the sexual season is shown to vary with the locality, both in regard to the number of dicestrous cycles in a season, and to the duration of each cycle. It is shown further that there is a perfect gradation between the monœstrous condition of some wild sheep to the extreme polyœstrum of certain Merinos.

Superficial Phenomena of Prœstrum and Œstrus.—The prœstrum is marked by a mucous or sanguineo-mucous flow. It is very rapidly succeeded by œstrus (the period of desire), the two periods frequently seeming to occur simultaneously, but this is because of the abbreviation of the process.

The Histology of the Uterus during the Dicestrous Cycle.—The changes through which the sheep's uterus passes during a single dicestrous cycle can be divided into four groups or periods, as follows:—(1) Period of rest; (2) Period of growth and increase of vessels; (3) Period of breaking down of vessels and extravasation of blood; (4) Period of recuperation and pigment formation. Bleeding into the uterine cavity and at the external genital aperture does not always

occur. The extravasated blood retained in the mucosa forms pigment, the change being effected by the agency of leucocytes, as supposed by Bonnet, but not by Kazzander. But the extravasation occurs in the superficial part of the mucosa. Miniature lacunæ are sometimes formed after extravasation. The severity of the procœstrous process tends to diminish with each successive diœstrous cycle in a season. There is never any removal of stroma and not necessarily of epithelium.

The facts recorded render the homology between the diœstrous cycle in the sheep and the menstrual cycle of the Primates very probable, while further statements regarding the author's researches on the ferret, with which the procœstrum in regard to severity is intermediate between that of the sheep and menstruation in monkeys, afford additional evidence of the identity of the two processes.

Ovulation, with Notes on the Atretic Follicle and the Causes of Barrenness.—Ovulation can occur spontaneously at any œstrous (or procœstrous) period with Scotch black-faced sheep, excepting at certain œstri outside the regular sexual season, when the additional stimulation supplied by coition may be necessary. In the ferret, ovulation does not occur in the absence of coition, without which the follicles undergo atresia. In the sheep, atresia is commonest in follicles of about one-eighth to one-half the dimensions of the mature follicles. When it occurs with any considerable frequency, it must affect the barrenness percentage in subsequent breeding seasons. The atretic follicle differs from the developing corpus luteum in the absence of any discharge to the exterior, the membrana granulosa degenerating and disappearing prior to any considerable ingrowth from the connective tissue wall.

The Formation of the Corpus Luteum.—The lutein cells are derived from the membrana granulosa, while the connective tissue element is supplied by the proliferation and ingrowth of the thecæ interna and externa, as described in the preliminary communication. Leucocytes are abundant, especially at the sixteen-hour stage of development, but these disappear in later stages without giving rise to connective tissue as described by Sobotta. The cavity of the discharged follicle is filled in by the further ingrowth of connective tissue.

"On the Culture of the Nitroso-bacterium." By H. S. FREMLIN, Lymph Laboratories, Chelsea Bridge. Communicated by Sir MICHAEL FOSTER, K.C.B., Sec. R.S. Received February 23,—Read March 12, 1903.

(Carried on at Westminster Hospital Medical School and the Jenner Institute of Preventive Medicine.)

My object in undertaking this work was in the first place to obtain a pure culture of the nitroso-bacterium; in the second place to discover whether it really was a fact that this species was unable to grow in the presence of organic matter, as stated by Winogradsky.*

My experiments with the nitroso-bacterium appear to show that:—

1. A practically pure culture of the bacterium can be obtained after sub-culturing for 7 months in Winogradsky's ammonia solution.

2. That the nitroso-bacterium will grow in this solution in the presence of organic matter.

3. That the nitroso-bacterium will grow not only on silica jelly, but also in any ordinary organic medium.

In the course of these experiments pure cultures were again and again obtained by plate cultivation from a great variety of artificial media. Single colonies therefrom were sub-cultured, and these were commonly competent to convert ammonia in a solution into nitrous acid. I infer, therefore, that there are not two separate and distinct species of bacterium, morphologically similar, and able to persist side by side indefinitely in inorganic solutions apart altogether from other bacteria, the one able to convert ammonia into nitrous acid and cultivable only in special media, the other growing on ordinary media but with no ability to convert ammonia into nitrous acid.

The purpose of my paper is to show that I have been dealing with a single species of bacterium, which is not only able to oxidise ammonia but is capable also of growing in ordinary organic media.

The solution used by Winogradsky for growing the nitroso-bacterium consists of water containing 1 per 1000 ammonium sulphate, 1 per 1000 potassium phosphate, and 1 per 100 magnesium carbonate. The carbonate solution is sterilised separately, and added to the solution of salts after sterilising, to prevent chemical decomposition.

This solution I have continued to use all through the work, and in this paper it will be referred to as the "ammonia solution." It has

* It was in 1895 that, in view of Winogradsky's work, I commenced these investigations. Since that date, and while my own research has been in progress, I have studied, in their bearing on the subject of my labours, a number of papers in various journals by a number of observers, whether in criticism or in support of Winogradsky's thesis, as will be seen on reference to my "full" paper.

always been tested for the presence of oxides of nitrogen, and a control set up when batches of the solution were inoculated.

The presence of nitrites was judged by a solution of diphenylamine in sulphuric acid, Ilosvay's solution being used as a control when necessary.

When I commenced work I obtained cultures of the nitroso-bacterium by inoculating ammonia solutions with small quantities, 0·2 gramme or less, of various kinds of soil; rich garden soil, humus, sand, &c. Tubes of ammonia solution so inoculated were kept at room temperature and placed in a dark cupboard in order to avoid exposure to light.

The evidence of the growth of the nitroso-bacteria was found in the conversion of the ammonia in the solution into nitrous acid. This is at first a slow process and does not commence in the tubes for some 3 weeks as a rule; usually it requires another week or two to be completed.

I inoculated ammonia tubes with soil 43 times. Of these 70 per cent. showed oxidation of the ammonia.

Gelatine plates poured from these tubes showed moulds, yeasts, liquefying and non-liquefying bacteria, and also a micro-organism morphologically similar to the nitroso-bacterium.

1*st Dilutions.*—From the ammonia tubes which showed oxidation sub-cultures were made in like media. Of these 77 per cent. showed nitrites. This occurred in 8 weeks.

2*nd Dilutions.*—From the first dilutions sub-cultures were made. Of these 85 per cent. showed oxidation in from 2 to 3 months.

3*rd Dilutions.*—From the second dilutions sub-cultures were made. All these showed formation of nitrite in one month.

4*th Dilutions.*—Sub-cultures from the third dilutions exhibited formation of nitrite in 93 per cent. of the tubes inoculated. These, as a rule, required 6 weeks before this was completed.

5*th Dilutions.*—Sub-cultures were made from the fourth dilution tubes. Of these 77 per cent. showed the formation of nitrite; the time required being 2 months.

With regard to the microscopical specimens made from the several tubes; the 1st, 2nd, and 3rd dilutions showed, associated with the nitroso-bacterium, rod-shaped micro-organisms, in gradually diminishing numbers in succeeding dilutions. The 4th and 5th dilutions gave almost pure culture of the nitroso-bacterium apparently.

Ammonia Solution containing no Carbonate.

A medium containing simply ammonium sulphate and potassium phosphate was tried.

The nitroso-bacterium was able to grow in this, and to produce nitrite; but the micro-organism did not develop in sub-cultures.

Liquid Media containing Organic Matter.

I made a series of experiments with "ammonia solutions" containing peptone beef broth, Witte's powdered peptone, and urea.

Beef Broth.

This was added in quantities varying from 1 in 11000 to 10 in 100.

Cultures of nitroso-bacteria grew well when inoculated from inorganic solutions into the lower percentages of beef broth, and on transferring them to higher percentages they were able to continue their nitrification. If cultures of nitroso-bacteria were taken from inorganic solutions and placed directly into solutions containing beef broth to the extent of 1 in 1000 they failed to show oxidation.

Peptone.

This was used in solutions containing 1 in 11000 and 1 in 5000.

The nitroso-bacterium was able to grow in these solutions; its development being satisfactory, as shown by the formation of nitrite.

Urea.

"Ammonia solutions" were prepared which contained from 1 in 11000 to 1 in 1000 of urea.

The nitroso-bacterium although developing in the presence of small quantities of urea failed to do so when the solution contained as much as 1 in 1000.

The above experiments show that the nitroso-bacteria can grow in the presence of organic matter. The addition of small quantities of organic matter to ammonia solution containing the nitroso-bacterium does not apparently check the formation of the nitrous acid.

The addition of larger percentages of organic matter to "ammonia solutions" does tend to check and finally stop the action of these bacteria if they are introduced directly from soil, or from inorganic media.

The experiments also show that, where nitroso-bacteria have oxidised ammonia in solutions which contain small quantities of organic matter, they are able to continue this work when transferred to solutions of ammonia containing amounts of organic matter that entirely arrest their oxidising action when they are transferred thereto direct from inorganic solutions.

Isolation of the Nitroso-bacterium.

Plate Cultures.

In carrying out the work of isolation of the nitroso-bacterium I made plate cultures containing silica, gelatine, and agar media respectively.

Silica Plates.

I found that numerous species of micro-organisms grew on this medium, and that, therefore, it was necessary to use what seemed to be pure cultures of the nitroso-bacterium, as this species does not form colonies rapidly, and is liable to be smothered by the more quickly-growing bacteria if one attempts to isolate the nitroso-bacterium from soil or a very impure culture. The nitroso-bacterium grows well in this medium, and in one instance I was able to remove a single colony which oxidised the ammonia in a solution. From this culture inoculation was made into beef-broth agar, and plates poured. These plates grew large numbers of colonies in pure cultures.

Numerous colonies were taken from silica plates and grown on beef-broth agar, but such growth transferred to ammonia solutions did not produce nitrification. This being the case I thought that probably the micro-organism had lost its power of oxidising the ammonia, so that I tried to devise a means by which this function might be re-established; and for this purpose the micro-organism was placed in as natural surroundings as were attainable. The following was the method adopted. A single colony was taken from silica plate and inoculated on to sloping beef-broth agar. After growing there it was transferred to a sterile ammonia solution; this was allowed to filter daily through sterile soil, thus allowing of aeration of the growth whilst in its natural surroundings. This experiment succeeded, nitrite being formed in 10 weeks. A control filter showed no change.

Gelatine Plates.

Winogradsky states that the nitroso-bacterium does not grow on gelatine; so that, in the first place, the method that he advocates to obtain a pure culture of nitroso-bacteria was adopted.

Particles of magnesia were removed from an oxidised ammonia solution and sown on to gelatine plates. Now if these particles carried nitroso-bacteria alone there would, if Winogradsky be correct, be no growth, and such a particle showing no growth could be removed and reinoculated into an ammonia solution, and thus a pure culture obtained. But I found that around particles so inoculated into gelatine, colonies invariably occurred. It was noted that these colonies were in pure culture and were made up of an oval organism that was morphologically similar to the nitroso-bacterium.

Secondly, gelatine plates were poured from oxidised ammonia solutions; these also gave the same species of micro-organism, often in practically pure culture. From one such plate a piece was removed, and in an ammonia solution it produced oxidation. This oxidised ammonia solution again yielded the same species on gelatine plates.

Gelatine media prepared from divers soils and inoculated with oxidised ammonia solutions exhibited the same species of micro-organism.

Hence it is to be inferred that, either the nitroso-bacterium grows on gelatine, or that an organism morphologically similar occurs in the same inorganic solutions with the nitroso-bacterium and thrives like it in inorganic solutions.

To arrive at some definite conclusion on this point further experiments were made with agar plates.

Agar Plates.

In commencing my researches with Agar, beef-broth agar was used.

Beef-broth Agar.

Plates of this medium, inoculated from cultures containing nitroso-bacteria gave similar results to those with gelatine; that is to say, micro-organisms, morphologically similar to the nitroso-bacterium, grew well on agar, as they had done on gelatine.

Pieces of these plates showing colonies were on fifty-three occasions inoculated into ammonia solutions; of these fifty-three solutions twenty showed formation of nitrite.

Pieces of such plates, on which no colonies were found, were in nineteen instances inoculated into ammonia solutions. In no case did the formation of nitrite occur.

Hence we have:—

Agar Plates with Colonies.

Inoculated.	Oxidised.
53	20

Agar Plates without Colonies.

Inoculated.	Oxidised.
19	0

I also made numerous experiments with a medium which I term "ammonia agar." This consists of:—

Ammonium sulphate, 1 gramme.
Potassium phosphate, 1 gramme.
Distilled water, 1 litre.

The salts are dissolved, and agar added to 1½ per cent.; the whole being boiled up and prepared as ordinary agar. Magnesium carbonate is added after sterilisation.

As will be seen, this agar corresponds in composition to the ammonia solution used for the ordinary cultures, save for the presence of the 1½ per cent. agar. It has a slightly lower melting and coagulation point than bouillon agar.

I have poured over 100 plates of this medium. It grows the nitroso-bacterium well, oxidation of the ammonia in the plate occurring within 2 months, as a rule, after inoculation with oxidised ammonia solutions.

All plates that showed oxidation of the ammonia contained large numbers of colonies of apparently the same bacterium. This organism being oval in form, and associated with the formation of nitrite, and being often almost or altogether in pure culture, must be considered to be the nitroso-bacterium. It occurred in the dilution plates in all instances. Nevertheless but few of these showed oxidation of the ammonia.

The following table gives results obtained :—

	Plates, number poured.	Number showing oxidation.
Original	26	22
First dilution.....	26	3
Second dilution.....	26	1

This shows that unless the colonies were numerous nitrite was not formed.

In one instance a single colony taken from an ammonia agar plate and placed on sloping ammonia agar formed nitrite in 9 months. Plates of beef-broth agar and gelatine poured from this culture grew enormous numbers of the colonies. These colonies develop both at room temperature and 37° C. At room temperature, after 6 days, the colonies appear to the naked eye as white iridescent growths varying in size. Some days later they become lemon coloured, and finally yellow. Under the microscope they were seen to be made of micro-organisms corresponding to the nitroso-bacteria.

"The Statolith-theory of Geotropism." By FRANCIS DARWIN, M.A., M.B., F.R.S. Received March 6,—Read March 12, 1903.

To make clear the point of view from which the experiments here recorded were undertaken, a few words on the modern theory of gravitational sensitiveness are necessary. For fuller details the reader is referred to the papers of Noll, Němec and Haberlandt,* to whom the theory in question is due.

When a geotropic organ suffers a change of position it responds by an appropriate curvature, and ultimately reassumes verticality. With the mechanism of curvature I am not concerned, the problem to be solved is the source of the stimulus to which the curvature is a response. In other words: when a plant, which normally grows vertically upwards, is laid horizontally, it obviously perceives the change; by what mechanism is the perception rendered possible?

The view of Němec and Haberlandt is that the stimulus is due to the presence of bodies of greater specific gravity than the cell sap, which in consequence of their weight always fall to the physically lower regions of the cells. These bodies are usually movable starch grains, and may conveniently be termed *statoliths*. As long as an organ is vertical, *i.e.*, in its normal position, the statoliths, in consequence of their weight, collect in a heap or layer on the basal walls of the cells. When the organ is placed horizontally the heap of statoliths topples over and gradually spreads out in a layer on the lateral walls, which are now horizontal. It is clear that in this circumstance is to be found the possibility of an appropriate stimulus. We can conceive that the pressure of the statoliths on the protoplasm of the lateral instead of the basal walls, might become associated with certain definite curvatures. Thus in a typical stem-structure the reflex action correlated with pressure on the lateral cell walls would be an upward curve, in a typical root the same stimulus would result in a downward curve.

The starch grains or other heavy bodies have been named statoliths in harmony with the nomenclature of the zoologists, who have given the name to the heavy particles which supply certain animals (*e.g.*, the Crustacea) with the means of orienting themselves spatially. The beautiful experiment of Kreidl† has definitely proved

* Noll, 'Heterogene Induction,' Leipzig, 1892.

Haberlandt's and Němec's papers were published simultaneously in vol. 18 (1900) of the 'Berichte der Deutschen Botanischen Gesellschaft.' See also papers by both authors in vol. 20 of the same Journal; also Němec, in 'Pringsheim's Jahrbücher,' vol. 36, 1901, and Haberlandt in the same Journal, vol. 38, 1903.

† 'Sitzb. Wiener Ak. d. Wiss.,' vol. 102, 1893, Abth. 3.

that the Crustacean *Palæmon* is guided in keeping its dorso-ventral plane vertical, and dorsal surface upwards, by the stimulus of the statoliths resting on the internal surface of its otocysts.

In favour of their theory, Němec and Haberlandt have stated fully and well the general argument based on the distribution of statoliths. Thus, broadly speaking, statoliths are found to occur (in Phanerogams at least) in organs and parts of organs capable of gravitational stimulation. On the other hand, speaking generally, statoliths are absent where there is no power of curving geotropically. Thus in the older parts of stems, which have lost the power of curvature, the statoliths disappear from the endodermis.*

The presence of statoliths in the tip, and only in the tip of the root, is a striking fact, and one that is in general accordance with the theory set forth in the *Power of Movement in Plants*, and confirmed since the appearance of that work by more than one observer.† The same thing is true of the cotyledon of *Setaria* and *Sorghum*, the statoliths are practically confined to this part of the seedling in which the gravitational sensitiveness has been shown to reside.‡ For further details of the general argument from distribution we must refer to the writings of Němec and Haberlandt. Personally, I consider the general argument to be highly convincing, but this opinion seems not to be shared by others, and in any case it does not permit us to neglect experimental tests.

Experimental Evidence.

Němec lays some stress on the loss of geotropism following the destruction of the statoliths. He employs Pfeffer's method of embedding a seedling bean or pea in liquid plaster of Paris. In this way, when the gypsum sets, the seedling is prevented from growing, and for some unknown reason the treatment leads to the disappearance of the starch from the group of specialised cells at the tip of the root. According to Němec seedlings so treated fail to respond to gravitational stimulus. But the argument is not complete, it should be shown that the starchless roots are not also inhibited in their reaction to other stimuli. For instance, supposing the experiment was performed on seedlings of *Sinapis*, it should be ascertained whether the starchless radicles would or would not curve apheliotropically, while remaining incapable of geotropic curvature.

I have tried a similar experiment on *Setaria* and *Sorghum*. It was discovered accidentally that the cotyledons lose a great part of their

* Haberlandt, 'Pringsheim's Jahrbücher,' vol. 38 (1903), p. 451; and 'Ber. der Deutschen Bot. Ges.,' vol. 18 (1900), p. 264.

† Pfeffer, 'Annals of Botany,' vol. 8 (1894), p. 317; Czapek, 'Pringsheim's Jahrbücher,' 1895; F. Darwin, 'Linn. Soc. Journal,' vol. 35, 1902, p. 266.

‡ F. Darwin, 'Annals of Botany,' vol. 13, 1899, p. 567.

starch if kept for periods varying from 6—26 hours at a high temperature, *e.g.*, 33—38. Such seedlings show a marked loss of geotropic movement.

Exp. 201, March 3, 1902.

Eight specimens of *Sorghum vulgare* were kept for 21¾ hours at 37°, and others at 16·5°—19° C. They were then allowed to curve geotropically for 20h. 20m. when the "cool" lot showed an average curvature of 49·8°, while the "hot" specimens gave an average of 9°.

Exp. 204, March 4, 1902.

Twelve specimens of *Sorghum vulgare* in two equal lots for 21h. 5m. at 37° and at about 18°. Microscopic examination at the close of the experiment showed considerable irregularity in the disappearance of starch, and the following table shows that the degree of geotropic curvature is more or less parallel to the amount of starch remaining.

1. }	Starch in small quantities.....	72° (geotropic curve).
2. }	,, ,, ...	70°
3.	Less starch than 1 and 2	33°
4. }	Starch almost disappeared	20°
5. }	,, ,, ...	28°
6. }	,, ,, ...	11°

At first I was inclined to see in facts like these a striking confirmation of the statolith-theory, but experiments of the type of the two following prove conclusively that such an opinion is not justifiable.

Exp. 215, March 13, 1902.

Sorghum nigrum nearly de-starched by 6 hours at 40° C. On the following day exposed to incandescent gas for 4 hours.

Result—Cool, 40°·2 (average curvature to light).
Hot, 10°·6 ,, ,,

Exp. 223, March 26, 1902.

Sorghum nigrum nearly de-starched by 5h. 35m. at 33°—34° C. Part of them were then placed horizontally in the dark for 4½ hours, the remainder being vertical and exposed to incandescent gaslight for the same period.

Result.—Geotropism (average).....	Cool 47°	Hot 7°·2
Heliotropism ,, ...	,, 21°	,, 7°·7

These and other similar experiments showed us that we had no right to conclude that the loss of geotropic capacity depended on the absence of the special mechanism (statoliths), but rather that the loss of the starch may perhaps be no more than a symptom of exhaustion which shows itself both geo- and heliotropically.

Němec has shown that in decapitated roots, *i.e.*, roots from which

the tip containing the statocytes have been removed, the capacity for geotropism returns with the regeneration of the statoliths. Even here—though the fact is a striking one—the argument seems to require the heliotropic test. For obviously the regeneration of the tip may mark the recovery of a generalised sensibility, and not merely the rehabilitation of the special gravitational mechanism. The same objection holds to some extent with regard to Haberlandt's* experiments on plants found to be starchless in winter. It should have been more definitely shown that they are heliotropically active though incapable of gravitational reaction.

The Tuning-fork Method.

In the autumn of 1901, I began a series of experiments by a method which was at the time a new one, but has been in the meanwhile published by Haberlandt.† It seems to me that Haberlandt's argument is open to the objection above set forth, and as it is an objection I have tried to meet, my results seem to be worth giving.

My point of view was that if gravitational sensitiveness is a form of contact-irritability (which must be the case if the pressure of the statoliths on the plasmic membrane is the critical event), then it might be possible to intensify the stimulus by vibration. I hoped, by applying vibration in a vertical plane to a horizontal seedling, to make the starch grains dance on the lateral walls, and by such repeated blows on the protoplasm to produce more active geotropic response.

The experiments were made with seedlings of *Sorghum*, *Setaria*, and *Panicum*. In the earlier trials entire seedlings were used, but they were found difficult to fix horizontally with sufficient accuracy, and I consequently employed cut hypocotyls cemented by means of melted cocoa-fat on cork supports, and kept damp in small metal boxes, each containing a strip of wet filter-paper.

The vibration was supplied by means of a tuning fork driven by an electric escapement. The fork was fixed in a horizontal plane so that the vibration was vertical. The amplitude of the vibration varied in different parts of the fork from 4 mm. to less than a millimetre. The rate was about 47 vibrations per second.

The general plan of the experiments was to attach a pair of metal boxes, one to each limb of the fork, each box containing four to six seedlings fixed approximately horizontal. Control boxes were placed on a support a few centimetres from the fork. It was found essential to insure an identical temperature for the experimental and control plants. The fork is set in motion and the experimental plants sub-

* Haberlandt, 'Pringsheim's Jahrbücher,' 1903, p. 472.

† *Loc. cit.* (1903), p. 489.

jected to vibration for about 8—20 minutes. The control and experimental boxes are then placed on a klinostat to avoid further gravitational stimulus, and the angular curvature estimated after a few hours. The general result is clear, viz., that the plants subjected to vibration are more strongly curved. In other words, that vibration increases the gravitational stimulus.

Precautions and Sources of Error.

In performing the experiment it is necessary to take scrupulous care that the control and experimental plants receive similar treatment. In the case of both it is necessary to cement the plants into their boxes without allowing any possibility of geotropism being induced before the boxes are fixed to the tuning fork. In all the later experiments the plants were cemented into their places vertically and received no gravitational stimulus until horizontal on the fork, or on the place prepared for the control plants, as the case might be. Any error arising for the want of this precaution in the earlier experiments was equally divided between the experimental and control plants.

The most serious source of doubt and error is the tendency in the grass seedlings to nutate in various directions; all that can be said is that the error in question is equally applicable to the experimental and control plants. *Sorghum saccharatum* is especially liable to nutation, and had to be discarded as material.

A theoretical source of error arises from the fact that the movement of a vibrating rod being part of a curve there must be a generation of centrifugal force parallel to the rod and towards its free end. If the force were sufficient it would cause a displacement of the starch in the cotyledons of the grass seedlings, and this would affect those seedlings whose apices were directed towards the free end of the vibrating rod, precisely as if they were in an oblique position, the apices being downward. Now this position is known* to be more favourable to geotropism than horizontality, therefore the plants on the vibrating rod could not be fairly compared with the motionless specimens. But we were unable to see any displacement of starch in seedlings exposed for short periods to the vibration of the tuning fork. By a rough method I estimated the centrifugal force in my experiments as about 0·2 g. This is a force quite sufficient to affect the starch grains, if enough time is allowed; but it is hard to believe that for periods of 8—20 minutes it could have any serious effect. However this may be, I avoided, in the later experiments, all possible favouring of the experimental plants, by placing them with their apices towards the base of

* Czapek, 'Pringsheim's Jahrbücher,' vol. 27, 1895, p. 283.

the tuning fork. In this case any acceleration in the line of the fork would be in favour of the "still" specimens.

Table I requires a few words of explanation. The third column gives the number of the experiment and simply refers to the original notes. The next column gives the time in minutes during which the plants were left horizontal. Then follow the temperature and the length of time during which the plants were left on the klinostat. When two readings occur under this column it means, of course, that the curvature was measured more than once: thus in experiment 74 the curvature was read after 4 hours' rotation, and again after 6h. 33m. from the time at which the plants were originally placed on the klinostat. The column "Shaken" gives the angular deviation from the horizontal of the plants which had been on the fork; their average curvature follows in a separate column. The last two columns give in the same way the actual observations and the average for the control plants. For the sake of brevity we use the terms "Shaken" and "Still" for the experimental and control plants. In columns "Shaken and "Still" the letter x occurs occasionally in brackets, thus (x): this means that nutation downwards or sideways had occurred. In the same way (5) indicates a downward or lateral nutation of 5°. In striking averages, I have counted the nutating specimens and also those which showed no geotropism, *i.e.*, remained horizontal. Thus, if the readings were 13, 15 (x) 17, the average would be $\frac{45}{4} = 11{\cdot}3$: or if it had been 13, 15, 0, 17, the average would have been the same. When the cases of nutation were very frequent, or where the curvature occurred very slowly, as in experiments 131, 140, I have omitted the average curvature from consideration, although I have allowed the readings to remain in the table.

The first thing that strikes one is that the *shaken* specimens show a greater curvature than the *still* ones in a large majority of cases.

Thus, taking the whole of the thirty cases in which the average curvatures are calculated, we have:—

Shaken, curvature greater in	25 cases.
,, equals or practically equals *still*	3
Still, greater than *shaken*	2

Out of these thirty cases, seven are second readings, *i.e.*, records of curvature in experiments in which readings had been taken a few hours previously. If these are omitted, we get:—

Shaken, curvature greater.....	19
,, equals *still*.....	2
Still, curvature greater	2

Finally, there are four cases in which the plants were left on the fork

Table I.—Geotropism.

	Plant.	Ex.	Stim.	T.	Klino.	Shaken.	Av.	Still.	Av.
1901.			m.		h. m.				
Nov. 27...	*Sorg. vulg.*.	67	75	20	1 15	26 13 6 18 18 10 30 40 0 23 45 40	22·4	35 25 10 25 18 10	20·5
„ 28...	*Sorg. vulg.*.	72	40	..	6 6	10 33 38 42 40 76 43 30 22 25 47 65	39·2	15 0 30 41 30 40	26
„ 30...	*Sorg.*.....	74	20	19	4 0	10 12 30 12 25 12 23 8	16·5	20 0 7 12 0 0	6·5
					6 33	15 15 33 26 30 8 30 30	23·4	28 5 12 8 0 18	11·8
Dec. 2...	*Sorg. vulg.*.	75	20	..	4 1	42 20 8 16 (8) 0	..	25 14 (80—90 lateral)	
„ 3...	*Sorg. sacch.*	79	30	..	5 49	(10 10 30) 10 10 6	..	10 2 2 0 15 10	
„ 20...	*Sorg. vulg.*.	85	20	..	3 56	10 8 45 28 .. .. 18 22 65 12	26	13 (8) 6 7 .. .. 5 8 15 10	8
„ 9...	*Sorg. vulg.*.	86	20	22—23	3 31	15 0 0 0 .. .. 40 13 10 10	11 ..	20 50 0 0 .. .. 0 (10) 15 (5)	10·6
„ 20...	*Sorg. vulg.*.	115	82	20	4 10	9 14 0 15 7 11	9·3	6 6 10 (13 20) 16	6·3
„ 21...	*Sorg. vulg.*.	117	60	21—22	4 10	25 35 40	33·3	17 20 17	18
1902.									
Jan. 11...	*Sorg. ital.*..	128	25	21	3 40	15 25 10 .. .. .. 32 40 15	22·8	0 20 18 .. .. .. 18 30 22	18
„ 16...	*Sorg. nigr.*.	130	65	19·5	3 40	28 10 8 8 .. .. 8 5 0 3	8·8	12 12 (15) 11 .. .. 13 (4) 5 0	6·6
„ 15...	*Sorg. nigr.*.	131	24	19·5	3 18	10 0 0 0 .. .. 10 0 0 0	.. ..	8 8 0 0 .. .. (7) 0 0 0	
					22 28	30 18 26 25 .. .. 0 22 12 22	19·4	15 17 10 23 .. .. 3 4 10 18	12·5

Table I—*continued.*

	Plant.	Ex.	Stim.	T.	Klino.	Shaken.						Av.	Still.						Av.
1902.			m.		h. m.														
Jan. 18...	*Set. ital....*	133	20	21	7 21	80	75	82	69	..	..	71·1	65	52	80	28	..	..	51·4
						70	68	85	40	..	..		90	68	0	28	..	..	
„ 20..	*Set. ital....*	134	15	19	5 44	40	95	85	80	..	..	76·2	80	45	80	80	..	..	75·1
						115	75	80	40	..	..		95	63	68	90	..	..	
„ 22...	*Set. ital....*	136	10	20	4 55	30	30	20	10	..	..	28·2	32	12	12	14	..	..	30·9
						15	35	51	35	..	..		40	50	20	67	..	..	
„ 23...	*Set. ital....*	140	5	..	5 13	5	(*x*)	(*x*)	0	..	..	..	11	9	(*x*)	1	..	..	
						11	(1)	..	..	..	..	..	(2)	5	0	0	..	..	
„ 24...	*Set. ital....*	142	8	..	3 27	23	15	23	25	..	..	25·1	14	13	14	0	..	..	15·5
						36	53	19	7	..	.		19	22	26	16	..	..	
„ 27...	*Sorg. vulg..*	144	8	19	3 11	5	8	0	(*x*)	..	..	5	5	5	10	1	..	..	7·3
						2	5	8	12	..	..		(*x*	*x*)	10	27	..	..	
					6 49	17	25	12	0	..	..	14·8	0	35	20	15	..	..	14
						13	10	31	10	..	..		(*x*)	0	5	37	..	..	
„ 28...	*Set. ital....*	149	8	18	3 6	15	0	0	15	..	..	3·8	(*x*)	11	9	(*x*)	..	..	2·5
Feb. 5...	*Sorg. vulg..*	167	9	..	3 15	11	8	13	5	..	..	11·4	2	9	9	6	..	..	6·6
						9	12	21	12	..	..		13	6	4	4	..	..	
					7 10	22	17	25	10	..	..	17·5	4	10	19	9	..	..	10·8
						10	22	20	14	..	..		18	12	10	4	..	..	
Feb. 6....	*Set. ital....*	170	8	17	2 35	8	12	18	2	..	..	10·1	5	7	0	0	..	..	3·0
						10	14	10	7	..	..	..	9	3	0	(*x*)	..	..	
					7 20	18	21	27	15	..	..	17·4	8	10	3	0	..	..	5·5
						10	15	20	13	..	..	..	15	8	0	(*x*)	..	..	
„ 7....	*Sorg. nigr...*	172	8	18	3 38	14	12	15	25	..	..	13·6	(28)	(10)	0	0	..	..	0·5
						8	18	10	7	..	..	..	(8)	4	(2)	(8)	..	..	
„ 13....	*Set. .....*	183	8	..	2 30	2	6	7	8	..	..	5·8	(5)	8	2	2	..	..	2·8
													0	0	0	10	..	..	
„ 14....	*Set. .....*	184	10	18	3 48	(3)	0	12	0	..	..	3·0	6	(15)	1	3	..	..	2·5

Table I—*continued.*

	Plant.	Ex.	Stim.	T.	Klino.	Shaken.	Av.	Still.	Av.
1902.			m.		h. m.				
Feb. 14....	*Set. ital.* ..	185	10	18	3 20	5 12 12 20	7·9	0 3 2 0	2·5
						0 8 6 0	..	3 5 2 5	
					4 32	10 10 13 6	7·9	3 5 6 0	3·7
						0 10 4 (2)	..	3 7 2 0	
„ 18....	*Set. ital.*....	186	12	19	1 40	16 12 10 22	9·8	0 2 2 5	2·2
						8 0 10 (2)			
					4 33	13 9 19 26	11·6	5 5 13 12	8·8
						15 (2) 11 (5)			
„ 18....	*Set. ital.*....	187	12	19	1 2	32 7 17 17	12·9	6 11 1 1	5·8
						17 12 (1) 1	..	9 3 12 3	
					4 54	33 6 23 16	15·6	7 16 0 7	7·5
						28 17 (4) 2	..	10 7 11 2	

for long periods, *i.e.*, 40—82 minutes; these are, perhaps, not strictly comparable with the other experiments, though in three out of the four the *shaken* specimens were clearly more curved than the *still* ones. If these are omitted, we have:—

Shaken, curvature greater	15
„ equals *still*	2
Still, curvature greater.....	2

The amount of curvature is not merely greater in a large majority of cases, but also differs by a considerable percentage.

Thus, taking the sum of the average curvatures in all thirty cases, we get:—

Still.	Shaken.	
403°·7	600°·8	or 100 : 148·8

Omitting the seven second readings (which give the sums—*still* 62·1, *shaken* 108·2), we get:—

Still.	Shaken.	
341°·6	492°·6	or 100 : 144·2

Finally, omitting the four cases of long exposure (which give—*still* 71·1, *shaken* 103·7), we get:—

Still.	Shaken.	
270°·5	388°·9	or 100 : 143·8

In another series of experiments the difference between the *shaken* and *still* plants was much smaller. But we have reason to believe that the failure depended on the small amplitude of vibration employed, for when, in the last four experiments of the series, a more ample vibration was adopted, there was once more a well-marked increase in geotropism in the *shaken* specimens.

In none of our experiments have we seen such striking results as those obtained by Haberlandt; it must, however, be remembered that his apparatus differs from, and is apparently more effective than, ours.

Control Experiments (*Heliotropism*).

The experiments were made in the same way as the last, except that the seedlings were vertical instead of horizontal, so that the starch would be made to vibrate on the basal instead of on the lateral walls of the cells. The boxes had glass lids to admit the light, which was given by incandescent gas. Care was taken that the distance from the light of the *still* specimens was the same as that of the *shaken* specimens.

Table II.—Heliotropism.

	Plant.	Ex.	Stim.	T.	Klino.	Shaken.	Av.	Still.	Av.
1901.					h. m.				
Dec. 14...	*Sorg. vulg.* ...	102	70	18—19	0 0	10 15 0 15	10·0	5 20 10 15	12·5
					3 25	8 15 12 (6)	8·8	14 4 0 26	11·0
„ 17...	*Sorg. vulg.* ...	109	90	21—23	0 0	12 5 25 10	13·0	0 12 8 0	5·0
					4 30	22 (5) 30 15	16·8	5 17 20 0	10·5
„ 18...	*Sorg. vulg.* ...	111	50	22—23	0 0	8 15 0 0	5·8	5 30 0 10	11·2
					4 5	38 32 30 28	32·0	70 45 30 48	48·2
1902.									
Jan. 10...	*Set. ital.*	126	58	23	5 10	67 50 22 ..	46·3	45 30 38 ..	37·6
„ 9...	*Sorg. vulg.*....	127	65	22	4 5	25 30 12 ..	22·3	10 27 15 ..	17·3
Feb. 8...	*Sorg. vulg.*....	173	60	22	2 28	0 13 3 7	5·8	8 12 5 8	8·2
					5 38	(*x*) 15 0 10	6·2	10 14 9 12	11·2
„ 21...	*Set. ital.*	188	61	..	3 23	4 15 7 12 22	12·0	8 10 10 11 2	8·2
Oct. 7...	*Set. ital.*	224	40	21	4 40	26 48 18 33	28·3	22 26 (8) 9	22·3
						20 15 30 36	..	38 25 28 30	
„ 8...	*Set. ital.*	225	37	22	5 18	22 15 25 17	17·0	5 38 10 0	14·4
						27 11 19 (5)	..	(3) 15 33 14	
„ 9...	*Set. ital.*	226	35	21	5 19	90 10 72 45	57·5	78 73 65 38	57·0
						48 98 42 55	..	17 30 85 70	
„ 10...	*Pan. mil.*	227	32	22	5 33	19 31 27 18	20·9	20 8 5 11	14·0
						23 0 32 17	..	28 15 11 14	
				Total, omitting 1st reading in Exps. 102, 109, 111.....			273·9		259

In three of the experiments (102, 109, 111) readings were taken before the plants were placed on the klinostat, and these readings are omitted in summing up the results :—

Curvature of *shaken* plants greater...	7 cases
„ *still* „ „ ...	4
Shaken, equal *still*.....	1

Thus there is a majority of wins in favour of the shaken plants, though it is not nearly so striking as in the geotropic experiments. When the *amount* of curvature is taken into consideration, the result is decisive :—

	Still.	Shaken.
Sum of averages	259·9	273·9
or as	100	105·4

The conclusion at which I arrive is that vibration does not materially increase heliotropic curvature, whereas it does increase geotropism. The following figures show in round numbers the difference between the geotropic and heliotropic experiments :—

Geotropic Curvature.

Still.	Shaken.
100	144

Heliotropic Curvature.

Still.	Shaken.
100	105

The inference I draw from this result is the same as that of Haberlandt, viz., that the increased geotropism of the *shaken* specimens is due to the increased stimulus produced by the vibration of the starch grains on the protoplasm of the lateral walls of the cells.

I desire to express my thanks to Miss Pertz, and to my assistant Mr. Elborn, for much valuable help.

"On the Laws governing Electric Discharges in Gases at Low Pressures." By W. R. CARR, B.A., University of Toronto. Communicated by Professor J. J. THOMSON, F.R.S. Received February 11,—Read March 5, 1903.

(Abstract.)

The experiments described in this paper were undertaken with the object of determining the potential difference required to produce discharge in a number of gases over a wide range of pressures, and especially of ascertaining if the law enunciated by Paschen* was generally applicable, provided the electric field in which the discharge took place was uniform.

The paper is divided into the following sections :—

(1.) Introduction.
(2.) Description of apparatus.
(3.) Experiments in air.
(4.) Experiments in hydrogen.
(5.) Experiments in carbon dioxide.
(6.) Spark potentials with different electrodes.
(7.) Minimum spark potentials.
(8.) Connection between spark lengths and spark potentials.
(9.) Minimum spark potentials in different gases.
(10.) Summary of results.

Paschen's experiments showed that when a given potential difference was applied to two spherical electrodes whose distance apart could be varied, the maximum pressure at which discharge occurred in a gas varied inversely with the distance between the electrodes. The range of pressures covered by his experiments did not extend below 2 cm. of mercury.

While Paschen has shown that as the pressure of a gas diminishes the difference of potential necessary to produce discharge between electrodes in a gas, a fixed distance apart, also diminishes, Peace† has shown that a critical pressure is finally reached when the spark potential reaches a minimum value, and that below this critical pressure the potential difference required to produce discharge rapidly increases as the pressure is lowered. Peace's experiments were conducted with air, and his electrodes consisted of a pair of large parallel plates supported in the gas. The values of the spark potentials recorded by him led to the conclusion that Paschen's law did not hold for electric discharges at and below the critical pressure.

In this paper it is shown that with the apparatus used by Peace, the

* Paschen, 'Wied. Ann.,' vol. 37, 1889, p. 79.
† Peace, 'Roy. Soc. Proc.,' vol. 52, p. 99.

discharges at low pressures in all probability did not take place along the shortest path between the plates, and it is inferred that the failure of his numbers to establish the applicability of Paschen's law at all pressures is due to his having always taken this shortest distance between the electrodes as a measure of the spark length.

In the present paper an account is given of an investigation on the potential difference necessary to produce discharges in a gas with a form of apparatus which ensured the passage of the discharge in a uniform electric field at all pressures. With this apparatus the spark potentials were determined in air, hydrogen, and carbon dioxide, for different distances between the electrodes, over a range extending considerably above and below the critical pressures. Electrodes of brass, iron, zinc, and aluminium, of the same size, were in turn used in the apparatus, but the readings obtained showed that the spark potentials were not influenced at any pressure by the size of the electrodes, provided the discharge took place in a uniform field.

The result of the investigation not only confirmed the truth of the law enunciated by Paschen for discharges in a gas at high pressures, but also demonstrated the applicability of the same law to the critical and to lower pressures. This law is summarised in the statement "that with a given applied potential difference, electric discharge in a uniform field in any gas is dependent solely on the constancy of the quantity of matter per unit cross-section between the electrodes."

It is shown that a general application of Paschen's law demands that the minimum spark potential must be a physical constant for each gas. A special set of observations gave the following values of this quantity for a number of simple and compound gases.

Gas.	Minimum spark potentials in volts.
H_2	278
O_2	455
H_2S	414
CO_2	419
N_2O	420
SO_2	457
C_2H_2	467

Adopting Strutt's value of 251 volts for nitrogen, the conclusion is drawn that the minimum spark potential is a property of the atom rather than the molecule of a gas, and it is shown that if H′, N′, O′, etc., represent the spark potential constants in volts, corresponding to atoms of the gases H_2, N_2, O_2, etc., respectively, the minimum spark potential for any compound gas whose formula is $H_xN_yO_z$, etc., will be given by $xH' + yN' + zO' +$ etc. volts. It is pointed out that oxygen forms an exception to the general application of this law.

The latter part of the paper deals with the extension of Paschen's law to spark lengths much shorter than those actually used in the experiments, and evidence is adduced in support of the conclusion that the law is applicable for discharges in a uniform field in any gas, as long as the spark length is greater than the diameter of the sphere of molecular action.

"On the Optical Activity of Hæmoglobin and Globin." By ARTHUR GAMGEE, M.D., F.R.S., Emeritus Professor of Physiology in the Owens College, Victoria University, and A. CROFT HILL, M.A., M.B., late George Henry Lewes Student in Physiology. Received January 31,—Read February 12, 1903.

Introductory Observations.

All observations hitherto published concerning the optical activity of the albuminous substances have led to the conclusion that the bodies thus designated, whether derived from the vegetable or the animal kingdoms, without a single exception, deviate the plane of polarisation to the left, no case having hitherto been known either of a dextrogyrous, a racemic, or an otherwise inactive albuminous substance.*

There is one group of albuminous substances which, notwithstanding the fact that it includes bodies of paramount physiological and chemical interest, has hitherto been completely neglected, in so far as the investigation of the optical activity of its members is concerned. The group to which we refer is that which has been designated by German writers the group of the "Proteïde." This group comprises those complex albuminous substances which can, with greater or less ease, be split up into, or which yield as products of decomposition, on the one hand, albuminous bodies, and on the other, such bodies as colouring matters, or nucleins and nucleinic acids and the purin-bases which result from the decomposition of the latter. The best

* Whilst this paper was being printed, it has come to our knowledge that the late Professor Alexander Schmidt, of Dorpat, described under the name of Cytoglobin, what was certainly a mixture of impure nucleoproteids which he separated from the soluble constituents of many animal cells. He definitely recognised the dextrorotatory properties of this product. For information on A. Schmidt's work, the reader is referred to the "Supplementary Bibliographical Note" at the end of the paper by Gamgee and W. Jones "On the Nucleoproteids of the Pancreas' Thymus, and Suprarenal Gland, with especial reference to their Optical Activity." *Infra*, p. 385.—*March* 5.

characterised and the most striking members of this group are: Firstly, the hæmoglobins and their compounds. Secondly, the nucleoproteids and the nucleins.

In hæmoglobin, we have the example of a complex proteid, which differs from all other members of the albuminous group of bodies by its colour, by its marvellous power of forming easily dissociable compounds with oxygen and certain other gases, by the facility with which it admits of being crystallised and recrystallised and obtained free from all foreign mineral matters, by the startling manner in which its solutions fail to furnish any one of the reactions characteristic of albuminous substances in solution, *so long as the reagent has not effected a fundamental decomposition which has liberated the albuminous and coloured residues.* The researches of one of us have moreover lately shown that whilst hæmoglobin is a diamagnetic body, the iron-containing products of its decomposition by acids are not merely paramagnetic, but probably the most powerfully "ferromagnetic" organic bodies known to science.*

So complete a divergence thus exists in physical and chemical properties between hæmoglobin and the substances which are the immediate products of its decomposition, and which are doubtless linked together in it, that it appeared in the highest degree interesting to ascertain whether or not, in respect to optical activity, hæmoglobin would behave as an albuminous body proper and prove to be "lævogyrous." Having, if possible, determined this point, the subsequent step in the research would naturally be to determine the optical activity of the albuminous and coloured products of the decomposition of the hæmoglobin molecule.

1.—*Determination of the Optical Activity of Hæmoglobin.*

So far as the authors have been able to ascertain, the optical activity of solutions of coloured organic bodies has not yet formed the object of serious investigation. Landolt,† in the last edition of his authoritative work, which contains all the reliable results relating to the optical activity of organic bodies up to the date of its publication, mentions only one colouring matter as having been investigated, viz., the vegetable colouring matter hæmatoxylin, of which the alcoholic solution is said to be dextrogyrous. Nor is this neglect of the study

* A. Gamgee, "On the Behaviour of Oxy-hæmoglobin, Carbonic-oxide-hæmoglobin, Methæmoglobin, and certain of their Derivatives in the Magnetic Field, with a Preliminary Note on the Electrolysis of the Hæmoglobin Compounds," 'Roy. Soc. Proc.,' vol. 68, p. 503.

A. Gamgee, The Croonian Lecture for 1902, "On certain Chemical and Physical Properties of Hæmoglobin," 'Roy. Soc. Proc.,' vol. 70, p. 79.

† Dr. H. Landolt, 'Das Optische Drehungsvermögen Organischer Substanzen, &c., Zweite gänzlich umgearbeitete Auflage,' Vieweg u. Sohn, 1898.

of the optical activity of coloured solutions surprising when we consider the much greater difficulties which encounter the observer, in comparison to those attending the examination of colourless solutions.

The Method Employed in the Present Research.

When a powerful beam of white light is passed through a stratum 1 cm. thick of a solution of Oxy- or CO- hæmoglobin containing 0·9 per cent., the only region of the spectrum which is unabsorbed is that which extends from B to a little distance on the red side of D. It was therefore clear that the only light which could be employed in the work before us was monochromatic red light, and that in the place of one of the polarimeters commonly employed in our laboratories and whose adjustments only permit of their being employed with light of a definite wave-length (the half-shadow polarimeters of the Laurent type being adjusted for use with monochromatic sodium light), an instrument should be employed, the arrangements of which permit of observations with light of any desired wave-length.

In our first observations, we attempted to employ the lithium flame as our source of light, but we were unable to secure by this means either a sufficiently powerful or steady illumination. We subsequently employed, however, as a source of practically monochromatic red light, the light of an arc lamp which had traversed Landolt's filter for red rays.

This light filter consists of a double cell, each compartment of which has a depth of 20 mm. One compartment is filled with a solution of hexamethylpararosanilin, a substance sold commercially under the name of "Crystal Violet 5 BO." 0·05 gramme of this compound is dissolved in a small quantity of alcohol and the solution is then diluted with water to the volume of one litre. When light is made to traverse a stratum 20 mm. thick of this solution, its spectrum consists of a narrow red band and a broad blue-violet part. If, however, the second compartment of the double trough contains a solution made by dissolving 10 grammes of potassium chromate in 100 c.c. of distilled water, the blue-violet is entirely absorbed and the spectrum of the light which has traversed the two compartments of the light-filter consists of a narrow strip, extending from λ 718 to λ 639$\mu\mu$, where it ends abruptly. The mean wave-length ("optischer Schwerpunkt") corresponds to 665$\mu\mu$, the wave length of C being 656·3$\mu\mu$.*

By means of the above method we secured a beam of red light having a mean wave-length approximately the same as that of C and of sufficient intensity to allow us to make observations on solutions of hæmoglobin containing $\pm$ 1 gramme in 100 c.c. of distilled water, the tubes employed in different sets of observations being 100 mm. and 200 mm. in length.

* Landolt, *op. cit.*, pp. 387—390.

The polarimeter employed in these observations was a magnificent Lippich's "Halbschatten-Polarimeter," with tripartite field of vision, made by Schmidt and Haensch, of Berlin, and belonging to the Davy-Faraday Laboratory of the Royal Institution of Great Britain.

*The Hæmoglobin Employed.**

The solutions of hæmoglobin employed for the determinations of which the results will be given below, were prepared with oxy-hæmoglobin of remarkable purity which had been obtained from the blood of the horse by following the best of the methods (the third method) described by Zinoffsky.†

Two preparations of hæmoglobin made on a large scale and at the interval of some months one of the other were employed. The preparation employed to make the solution of Oxy-hæmoglobin had been crystallised three times, the product of each successive crystallisation having been many times washed with ice-cold distilled water of which the purity was controlled by determining its electrical resistance. This solution contained 2·446 grammes of hæmoglobin in 100 c.c. For polarimetric observations this solution was diluted with an equal volume of distilled water, the dilute solution examined containing, therefore, 1·223 gramme of oxy-hæmoglobin in 100 c.c.

The preparation employed to make the solution of CO-hæmoglobin had been crystallised four times. The crystals of each successive crystallisation had been subjected to washing with pure distilled water as stated above, the solution of the washed crystals of the fourth crystallisation having been saturated with CO. This solution contained 1·84 grammes of dry CO-hæmoglobin. For polarimetric measurement this solution was diluted with an equal volume of distilled water; the dilute solution contained, therefore, 0·92 gramme of CO-hæmoglobin in 100 c.c.

Hæmoglobin, whether in Combination with Oxygen or Carbonic Oxide, is Dextrorotatory.

A. *Oxy-Hæmoglobin.*

The diluted solution of Oxy-hæmoglobin, previously referred to, was employed. This solution, containing 1·223 gramme of hæmoglobin in 100 c.c., was thoroughly saturated with oxygen before being subjected to polarimetric observation.

* The part of Section 1 of this paper which follows has been recast, and the observations described under A and C added, since this paper was submitted to the Royal Society.—*March* 5.

† Zinoffsky, O., "Ueber die Grösse des Hæmoglobinmolecüls," 'Zeitschrift f. physiol. Chemie,' vol. 16 (1886), p. 23.

The tube employed in all the sets of observations measured 1 decimetre.

Three sets of observations were made.

	Observed angle.	Specific rotation $(\alpha)_C$.
1. Mean of first set of observations ...	$+0^\circ{\cdot}12$	$+9^\circ{\cdot}8$
2. „ second set of observations	$+0^\circ{\cdot}125$	$+10^\circ{\cdot}2$
3. „ third „ „	$+0^\circ{\cdot}1225$	$+10^\circ{\cdot}0$

From the above observations we conclude that the specific rotation of Oxy-hæmoglobin for light of the mean wave length of C, $(\alpha)_C = +10^\circ{\cdot}0 \pm 0^\circ{\cdot}2$.

B. *CO-Hæmoglobin.*

The diluted solution of CO-hæmoglobin, previously referred to, was employed. This solution contained 0·92 gramme of CO-hæmoglobin in 100 c.c.

Two sets of observations were made with this solution; in the first set a tube 1 decimetre long, and in the second a tube 2 decimetres long being employed.

	Length of tube.	Observed angle.	Specific rotation $(\alpha)_C$.
1. Mean of first set of observations	1 decim.	$+0^\circ{\cdot}098$	$+10^\circ{\cdot}65$
2. „ second „ „	2 „	$+0^\circ{\cdot}203$	$+11^\circ{\cdot}03$

Taking the mean of the two series of observations we obtain as the specific rotation of a solution containing 0·92 gramme of CO-hæmoglobin in 100 c.c.

$$(\alpha)_C = +10^\circ{\cdot}8.$$

When the feeble rotatory power of hæmoglobin is considered, the agreement between the results of the investigation of the rotatory power of Oxy- and CO-hæmoglobin must be considered satisfactory and as pointing to the conclusion that the molecule of oxygen or carbonic oxide in combination with hæmoglobin does not influence its specific rotation. The correctness of this conclusion has been established by direct experiment.

C. *The same Solution of Hæmoglobin saturated with O and with CO Compared.*

With the object of determining by direct experiment whether the dissociable combinations formed by O and by CO with hæmoglobin had any influence on its specific rotation, the solution of Oxy-hæmoglobin which served for three sets of observations recorded under A, and which contained 1·223 gramme in 100 c.c. of water, was again experimented with. One portion of this solution was saturated with

oxygen; another portion was agitated with pure CO so as completely to expel the oxygen from its combination with hæmoglobin and replace it by carbonic oxide. In this way were obtained two solutions of hæmoglobin identical in so far as the quantity of colouring matter which they contained, but differing in the fact that in the one case the hæmoglobin was in combination with oxygen and in the other with CO. The solutions were examined in tubes of the same length under the same conditions of illumination. The result was to show that the rotations were identical in the two cases, having the mean value represented by the specific rotation $(\alpha)_C = +10^{\circ}{\cdot}0$.

It is to be remarked that the observations recorded under A and C were carried out subsequent to those on CO-hæmoglobin recorded under B. In the case, particularly, of observations A, the intensity and steadiness of the monochromatic red light employed was, in consequence of the experience previously acquired, more satisfactory than in observations B. We are therefore inclined to consider the numbers expressing the specific rotation of hæmoglobin which we have obtained as the result of observations A to be most worthy of confidence. We do not pretend that these numbers may not need slight modification as the result of future work, though we believe that they are a very close approximation to the truth.

2.—*Determination of the Optical Activity of Globin.*

Preyer gave the name of Globin to the albuminous product of the spontaneous decomposition of hæmoglobin, without, however, being able to furnish any precise account of its properties, its chemical composition, or its relationship to other albuminous bodies. A comparatively recent investigation which we owe to Fr. N. Schulz,* and the results of which have been substantially confirmed by Ivar Bang,† has placed us in possession of valuable and suggestive facts concerning the main albuminous product resulting from the decomposition of hæmoglobin. He has shown that when a solution of hæmoglobin is decomposed by the addition of small quantities of hydrochloric acid, it yields, as main products, 4·2 per cent. of hæmatin and 86·5 per cent. of a characteristic albuminous substance for which he retains the name of globin. He has shown that this substance belongs to the class of "the Histons," so that it would have been preferable, in our opinion, if Schulz had applied to his new body such a name as "Hæmato-Histon," which would have indicated both its origin and its affinities.

Schulz's method of preparing globin, as described by him, is essen-

* Schulz, Dr. Fr. N., "Die Eiweisskörper des Hæmoglobins," 'Zeitschr. f. physiol. Chemie, vol. 24 (1898), p. 449.

† Bang, Ivar, "Studien über Histon," 'Zeitschr. f. physiol. Chem.,' 1899, p. 463.

tially as follows: to a solution of crystallised hæmoglobin, either prepared by Hoppe-Seyler's method or by the ammonium sulphate method, dilute hydrochloric acid is added in extremely small quantities, until a flocculent brown precipitate falls which is immediately dissolved by the slightest excess of acid. The solution then no longer exhibits the beautiful red colour of hæmoglobin, but has assumed a brown colour. Not only, remarks Schulz, has its colour changed, but a complete separation has occurred between the albuminous and coloured constituents of hæmoglobin. If to the solution, which has now a faint acid reaction, about one-fifth of its volume of 80 per cent. alcohol be added and the mixture be shaken with ether, the whole [*sic*] of the colouring matter is taken up by the ether, whilst the subjacent aqueous-alcoholic, perfectly clear solution contains the decolourised albuminous matters. Schulz gives particular directions as to the precautions which must be taken in order that the separation of the ethereal solution of the colouring matter should be complete, stating that a certain relation must exist between the proportions of water, alcohol, and ether, which must be experimentally determined in each case. By the above process there is obtained a more or less brownish-yellow solution, containing both alcohol and water and having a faintly acid reaction. On neutralising this solution with ammonia, a faintly yellow, coarsely flocculent precipitate falls. The latter is rapidly separated by filtration and then washed with water. When the excess of ammonia has been removed, the precipitate commences to dissolve in the wash water. At this stage, the precipitate is dissolved in water with the aid of a few drops of dilute acetic acid. Solution occurs rapidly and completely. The excess of acid is now removed by dialysis continued for some days, the dialyser being surrounded by distilled water. There is thus obtained a clear, odourless and tasteless solution of globin the reaction of which is perfectly neutral.

It is not our object to examine in this place the reactions presented by solutions of globin, and which have led Schulz to place it among the "Histons."

Before describing briefly the methods we employed to prepare the solutions of globin which we investigated optically, we desire to make certain observations on certain points in Schulz's statement. In discussing the quantity of dilute hydrochloric acid needed to effect the decomposition of hæmoglobin, he merely remarks that it is extraordinarily small ("Die zu der Spaltung erforderliche Menge von Säure ist ausserordentlich gering, &c."). We have determined the quantity of decinormal hydrochloric acid required to effect the decomposition of a solution of CO-hæmoglobin of known composition. As a result of very careful experiments with a solution containing 1·84 grammes dissolved in 200 c.c. of water, there were required

20 c.c. of decinormal hydrochloric acid to effect the complete separation of globin from the colouring matter.

We found that agitation with ether, unless repeated several times, fails to remove all the colouring matter which is capable of removal in this way. Further, we found that even when, as a result of agitation with ether, the aqueous-alcoholic solution of globin is of the faintest straw colour, on being neutralised with ammonia the precipitated globin, which is at first colourless, assumes a somewhat reddish tinge, and when subsequently dissolved in water faintly acidulated with acetic acid the solution is much more deeply coloured than the original aqueous-alcoholic solution.

The following is the precise method which we followed in preparing the solutions employed in our polarimetric determinations :—

100 c.c. of a solution of four times crystallised hæmoglobin, containing 1·84 grammes of the substance, was diluted with 100 c.c. of distilled water and treated with 20 c.c. of decinormal hydrochloric acid. 44 c.c. of absolute alcohol were then added to the liquid, which was placed in a stoppered separating funnel and thoroughly agitated with its own volume of ether. The aqueous-alcoholic liquid having been separated from the supernatant ethereal solution of colouring matter was twice more agitated with fresh quantities of ether. By proceeding as we have described, the separation of the solution of globin occurred completely after the first agitation with ether, and the solution after the third agitation only possessed a faint straw coloration. In certain cases, the globin was separated according to the method of Schulz by precipitation with ammonia, the flocculent precipitate being subsequently dissolved in very weak acetic acid. In this manner was prepared the solution of globin which served for the first set of determinations recorded below. As it was impossible to obtain in this way solutions sufficiently colourless to allow of their rotation to be determined satisfactorily for light of the wave-length of D, this was done as in the case of hæmoglobin for light of the mean wave-length of C. In the second set of observations, the rotation of the aqueous-alcoholic solution resulting from the decomposition of hæmoglobin, after thorough agitation with ether, was determined.

Globin a Lævorotatory Substance.

Preliminary observations having shown that solutions of globin are optically active and lævogyrous, the following sets of observations were made with the object of determining the specific rotation of solutions of the substance.

1. A solution of globin in distilled water, but containing a little acetic acid, was examined with the arrangement for red light, as was used in the case of hæmoglobin. The solution contained 2·4 grammes

of globin in 100 c.c. It exhibited in the most characteristic manner the reactions of globin.

The tube employed measured 1 decimetre. The angle of rotation (mean of many determinations) was $-1°{\cdot}30$.

From the above data, it follows that in the case of this feebly acid solution of globin, containing 2·4 per cent., the specific rotation $[\alpha]_C = -54°{\cdot}2$.

2. The faintly straw-coloured solution obtained by the decomposition of hæmoglobin by means of dilute hydrochloric acid, the addition of alcohol and repeated agitation with ether, was placed in a shallow capsule in a current of air for some hours and afterwards on the water-bath at the temperature of 40° C. In this way all the ether and some of the alcohol were expelled. The perfectly clear straw-coloured solution, which had a density of 987·4 at 16° C., contained 0·98 gramme of solid matter in 100 c.c.

Monochromatic sodium light was employed in the polarimetric observations. The tube employed measured 1 decimetre. The angle of rotation (mean of many determinations) was $-0°{\cdot}64$.

From the above data, it follows that in the case of this feebly acid, aqueous-alcoholic solution of globin, containing 0·98 per cent. of solids the specific rotation, $[\alpha]_D = -65°{\cdot}5$. It may be pointed out that the greater part of the discrepancy between the results of the polarimetric measurements of the solution of separated globin and of the solution now under discussion is to be explained by the difference in the wave-length of the light, of which the rotation of the plane of polarisation was determined in the two cases.

General Conclusions.

The following are the conclusions to which we have been led by the experiments described in this paper :—

1. Hæmoglobin is a dextrogyrous albuminous body.

2. Globin, which is the principal, or as we are inclined to believe, the only albuminous product of the decomposition of hæmoglobin by highly dilute hydrochloric acid under the conditions determined by Schulz and confirmed by our own observations, behaves as a normal albuminous substance, in respect to its influence on the plane of polarisation of light, *i.e.*, it is a lævogyrous body.

Whilst the conclusions above stated are beyond question correct, we wish it to be understood that the numbers expressing the specific rotation of the bodies which we have examined must be looked upon as very close approximations and may need revision in the case of hæmoglobin by determinations carried out with a more perfectly monochromatic and intense light than that which we have employed, and in the case of globin by working with the substance in a purer condi-

tion than is possible in the actual state of our knowledge of this body.

We hope to be able to carry out these further investigations, and to direct our attention to the optical activity of the coloured products of the decomposition of the hæmoglobin molecule, especially hæmochromogen and hæmatin and their coloured derivatives.

In conclusion, we have to express our thanks to the Managers of the Davy-Faraday Laboratory of the Royal Institution for the facilities which they afforded us in carrying on the optical part of our work.

"On the Nucleoproteids of the Pancreas, Thymus, and Suprarenal Gland, with especial Reference to their Optical Activity."

By ARTHUR GAMGEE, M.D., F.R.S., Emeritus Professor of Physiology in the Owens College, Victoria University, and WALTER JONES, Ph.D., Associate Professor of Physiological Chemistry in the Johns Hopkins University. Received February 9,—Read February 12, 1903.

PART I.—BIBLIOGRAPHICAL AND CRITICAL.

In a research in which one of us was associated with Dr. A. Croft Hill, it was discovered that Hæmoglobin is a dextrorotatory body, whilst the interesting Histon-like albuminous substance Globin, which is obtained by the splitting up of Hæmoglobin under the influence of highly diluted hydrochloric acid, and of which the characters, no less than the mode of preparation, have only been known since the researches of Fr. N. Schulz, is a normally lævogyrous albuminous body.

These interesting observations naturally suggested the probability that the Nucleoproteids might, like Hæmoglobin, prove to be dextrogyrous, and the research of which the first results are contained in this paper is the outcome of this idea. The hypothesis has been fully confirmed, as will be shown in the sequel, and it has thus been proved that some of the members of a group of albuminous bodies of great importance in the life-history of the organism, are dextrorotatory bodies.

The preparation of nucleoproteids of such purity and especially so free from contaminating colouring matters as to yield solutions sufficiently transparent and colourless for polarimetric work, was a necessary preliminary to our special researches, and has led to the discovery of many facts of interest in relation to the chemistry of the nucleoproteids.

Preliminary Remarks concerning the "Nucleoproteids" and "Nucleins" and the Sense in which the Latter Term is used in the Present Paper.

By the term Nucleoproteids, we designate complex, or rather compound, albuminous substances which are the constituents of the nucleated protoplasm of all the organs of the animal body, but especially of the ductless, as well as of the secreting, glands. These bodies are characterised by the large quantity of phosphorus which they contain, by the constant presence of iron, and by the fact that under the influence of heat, by the action of acids, of alkalies, but especially of pepsin and hydrochloric acid, acting at temperatures favourable to their action, they split up into albuminous matters, and into so-called true (to distinguish them from pseudo-) nucleins. The latter differ from the mother nucleoproteids which yielded them, by the fact that they result from the splitting-off of a fraction of the albuminous molecules which these contained in their pristine and native condition. These secondary, or we may say, degraded nucleoproteids, "the nucleins," contain all the phosphorus originally present in the mother substance.

By the action of caustic alkalies and heat, the nucleins yield as products of decomposition, albuminous matters, and the so-called "nucleinic acids," bodies which vary in composition in the different nucleoproteids, but which are characterised by the fact that when heated with certain mineral acids, they yield as products of hydrolysis (Kossel), one or more of the purin-derivatives long known as "the xanthine bases," Adenin (Amidopurin), Guanin (Aminooxypurin), Hypoxanthin (Oxypurin), and Xanthin (Dioxypurin), as well as in many cases a base called Thymin, $C_5H_6N_2O_2$, a derivative of Pyrimidine.* At the same time, the phosphorus is separated as phosphoric acid.

Kossel, to whose fine researches we owe the greater part of our knowledge of the nucleinic acids, advanced the hypothesis (based on the great variation in the quantities of the xanthine bases which result from the hydrolysis of nucleinic acids of different origins) that there are four nucleinic acids, each of which yields one of the bases only. This theory of Kossel appeared to gain important support from Ivar Bang's† discovery of guanylic acid, a nucleinic acid obtained by the action of solution of potassium hydrate on the nucleoproteids of the pancreas, and which, as its name indicates, yields on hydrolysis one only of the purin-bases, viz., guanine. This hypothesis does not

* Walter Jones, 'Zeitschrift f. physiol. Chem.,' vol. 29 (1900), p. 26; H. Steudel u. A. Kossel, 'Zeit. f. physiol. Chem.,' vol. 29 (1900), p. 303; H. Steudel, 'Zeitschr. f. physiol. Chem.,' vol. 30 (1900), p. 539; vol. 39 (1901), p. 241.

† Ivar Bang, "Die Guanylsäure der Pancreasdrüse und deren Spaltungsprodukte," 'Zeitschr. f. physiol. Chem.,' vol. 36 (1898), p. 133.

appear to be in unison with the facts known to us (Schmiedeberg, Levene, W. Jones and G. H. Whipple, T. B. Osborne and I. F. Harris).*

Hammarsten,† to whose researches on the nucleoproteids and their relations to the nucleins we owe much of our knowledge of these bodies, would restrict the term "Nucleins" to the albuminous compounds of the nucleinic acids which remain undissolved after prolonged digestion with pepsin and hydrochloric acid. But this limitation appears to us undesirable and unphilosophical, and we think that the term nuclein, which it is convenient to retain, both for historical and descriptive reasons, should be applied to designate all those bodies resulting from the splitting-off of some, but only some, of the albuminous molecules originally forming part of the more complex nucleoproteid mother substance. It is in this sense that we shall in this paper employ the term nuclein, it being understood that every nuclein is to be considered a nucleoproteid, inasmuch as it is a compound of an albuminous body with a nucleinic acid or acids.

The Researches of Hammarsten on the Nucleoproteids of the Pancreas.

In his most interesting and suggestive paper published in the year 1894, Hammarsten gave an account of two nucleoproteids which he had obtained from the pancreas.

The first of these bodies he designated proteid-α. He ascertained that this body which, being soluble in water, is present in cold aqueous extracts of the pancreas, is precipitated by acetic acid, and that when boiled its solutions yield a coagulated albuminous substance, the substance remaining in solution being presumably nucleoproteid-β. Although Hammarsten fully recognised that the first or α-body was the mother substance, and that proteid-β was only a product of its decomposition, he devoted his attention to the latter, being actuated by the following reasons:—In the first place, his object being at that time to study the non-albuminous products of the pancreatic nucleoproteid, it appeared to him more simple and wiser to take as the starting-point of the investigation a material containing less albumin. The chief ground, however, for leaving the more interesting nucleoproteid provisionally uninvestigated was the great difficulty of obtain-

* O. Schmiedeberg, 'Archiv f. experiment. Path. u. Pharmak.,' vol. 43 (1899), p. 57; P. A. Levene, 'Zeitschr. f. physiol. Chem., vol. 32 (1901), p. 541; W. Jones and G. H. Whipple, 'Amer. Jour. of Phys.,' vol. 7 (1902), p. 423. See particularly the recent paper by Thomas B. Osborne and Isaac F. Harris, "Die Nucleinsäure des Weizenembryos," 'Zeitsch. f. physiol. Chem.,' vol. 36, Heft 2 (September, 1902), p. 85.

† Olof Hammarsten, "Zur Kenntniss der Nucleoproteïde," 'Zeitschr. f. physiol. Chem., vol. 19 (1894), p. 19.

ing it in any degree pure, attempts at purification being attended with such loss that the yield was too small.

Hammarsten remarked that among the impurities most difficult to separate was the blood-colouring matter, as well as another colouring matter which he believed to be produced by the action of the air on the nucleoproteid itself. Further, another impurity adhering to the nucleoproteid was found by Hammarsten to be trypsin, which he was unable to separate from it. He remarks, indeed, that the proteolytic activity of the substance is so intense that in no other way could he obtain so powerfully acting a trypsin.

Having, for the reasons above stated, abandoned the study of the interesting mother-substance, his nucleoproteid-α, Hammarsten then directed his attention to the β-body. This body, he did not seek to obtain by the decomposition of the mother substance, of which it is a product, but by adopting the following method:—he boiled the finely comminuted and perfectly fresh pancreatic gland of the ox in water and obtained, after filtration, a perfectly clear, faintly yellow solution, to which he added, after cooling, from 1 to 2 parts of hydrochloric acid, or from 5 to 10 parts of acetic acid per 1000 parts of the liquid. In this manner he obtained an abundant, white, flocculent precipitate. He dissolved the substance thus precipitated in water, with the aid of the least possible quantity of alkali and reprecipitated it by adding an excess of acid. By repeating this process several times, the body originally precipitated was purified, so far as such a method can effect the purpose.

It must be clearly insisted upon that, as Hammarsten himself pointed out, the so-called nucleoproteid-β does not represent an original proximate principle of the pancreas, but is a nuclein produced from the original mother nucleoproteid (or nucleoproteids ?) by the action of boiling water. It is certainly in no spirit of detraction or want of respect for the eminent Swedish chemist, that we add the remark that the study of a nuclein to be satisfactory should, if possible, take as its starting point the pure mother substance, of which it is a product of decomposition, rather than the animal tissue which contains that substance. In the case of Hammarsten's nucleoproteid-β, one can at present only assert that it is a nuclein or a mixture of nucleins produced by the action of boiling water on the nucleoproteids, properly so called, existing preformed in the tissue of the pancreas.

These strictures notwithstanding, we have to point out the remarkably interesting facts which were discovered by Hammarsten in the course of the investigation under review. He made a series of ultimate organic analyses of different specimens of this nuclein, and showed that whilst its solutions when boiled with Fehling's solution gave no trace of reduction, the body when heated on the water-bath with dilute sulphuric acid, furnished a highly reducing substance.

Although unable to separate the reducing substance in a state of purity, he succeeded in preparing an Osazone of constant melting point and the characters of which agree with those of the osazone of a pentose, an observation which absolutely coincides with the researches of Kossel and Bang, which establish the presence of a carbohydrate nucleus in the nucleinic acids and the formation of pentoses when they are subjected to the hydrolytic action of dilute mineral acids and heat. Further, Hammarsten showed that when his nuclein was decomposed by heating with a 3 per cent. solution of sulphuric acid on the water bath, a crystalline sediment often separated which, after being purified, was analysed and shown to consist of guanine sulphate. Later, at Hammarsten's instigation, Ivar Bang, continuing the investigation, prepared from Hammarsten's nuclein, the nucleinic acid to which he ascribed the name of Guanylic Acid.

Part 2.—Experimental.

On the Nucleoproteid of the Pancreas and on Certain Characters of the Nucleins which are associated with, or derived from, it.

A. *The Nucleoproteid.*

Method of Preparation.

The finely divided pancreas of the pig was treated successively with 50 per cent. alcohol, 75 per cent. alcohol, and 95 per cent. alcohol, and finally subjected to the action of absolute alcohol and ether, with the object of dehydrating it. The material thus obtained was extracted with successive portions of a 5 per cent. solution of ammonium acetate, the united extracts were filtered, and the perfectly clear fluid was poured into four times its volume of weak alcohol. The precipitate thus formed was washed by decantation with a large amount of dilute alcohol, and finally dried with absolute alcohol and ether. The object of this series of procedures was to remove the colouring matter of the gland, which is somewhat soluble in dilute alcohol, more so in an alcoholic solution of ammonium acetate, but soluble to a very slight extent in an aqueous solution of ammonium acetate. These manipulations also remove a large amount of inorganic salts, and render the coagulable albuminous substances insoluble.

A 2 per cent. aqueous solution of this raw material had only a pale yellow colour, and it was found that it could easily be examined in a tube measuring 220 mm. with the polarimeter, monochromatic sodium light being employed. The polarimeter was a "Halbschatten-Polarimeter" made by Schmidt and Haensch of Berlin. *The result of the examination was to show that the solution contained a dextrorotatory substance. The solution, moreover, failed to give any indication of the presence of a reducing substance, even by prolonged boiling with Fehling's solution*, and

was found to be rich in material which yields xanthine bases on hydrolysis with sulphuric acid.

The main portion of the gland substance, purified by the processes above described, was treated with 20 parts of water, and to the filtered solution acetic acid was added, drop by drop. When a quantity of acid had been added sufficient to bring the amount of acid in the entire solution to 1 per cent., a well-defined white, flocculent, precipitate separated. This precipitate of nucleoproteid was separated by the centrifuge, suspended in water, and treated with an extremely dilute solution of ammonia, drop by drop, and the reaction of the liquid continuously tested with litmus. A very small amount of alkali was needed to neutralise the adherent acetic acid, when the solution became neutral and remained so until approximately twice as much ammonia had been used as had been required to completely dissolve the nucleoproteid. Evidently, the nucleoproteid is, at least, a dibasic acid, whose acid ammonium salt is soluble in water and neutral to litmus.

Purification of the nucleoproteid was effected by alternate solution in ammonia and precipitation with a minimal quantity of acetic acid. The final solution was poured into five volumes of 95 per cent. alcohol, washed repeatedly by decantation with excessively large quantities of 95 per cent. alcohol and ether, and then placed in an exsiccator over sulphuric acid.

Optical Properties.

1. A weighed amount of the nucleoproteid was suspended in water and dissolved by the addition of a trace of ammonia. The solution was made up to a definite volume with water, and examined polarimetrically:

Weight of substance (W) ...	1·006 gramme.
Volume of solution (V)	25 c.c.
Observed angle (α).....	$+3^\circ 4'$
Length of tube (L).....	200 mm.

$$[\alpha]_D = +38^\circ{\cdot}1.$$

2. The results of the above observation were confirmed by the examination of another preparation of nucleoproteid.

Weight of substance	0·500 gramme.
Volume of solution.....	25 c.c.
Observed angle	$+1^\circ 30'$
Length of tube	200 mm.

$$[\alpha]_D = +37^\circ{\cdot}5.$$

The solution was treated with an excess of acetic acid and the precipitate filtered off. The filtrate was found to be inactive.

B. *Nuclein accompanying, and probably resulting from, the Nucleoproteid.*

Method of Preparation.

The aqueous extract of the purified gland substance to which acetic acid had been added until it contained 1 per cent. of the latter, and from which the nucleoproteid had thus been separated, was treated with 20 per cent. acetic acid added a drop at a time. When the liquid contained 2 per cent. of the acid not the slightest precipitation had occurred. Continued addition of acetic acid, however, soon caused a turbidity, and when the acidity reached 5—6 per cent., a well-defined flocculent precipitate fell. This precipitate, which we shall call nuclein, was separated by means of the centrifuge and, at a great cost of material, was twice washed with water, the washings being separated by centrifugalising. The washed nuclein was suspended in water, and solution of ammonia added cautiously, one drop at a time; when the nuclein was completely dissolved, the reaction of the liquid was still acid to litmus. This solution was poured into four volumes of 95 per cent. alcohol, and the precipitated nuclein washed and dried by the methods described in the case of the nucleoproteid.

The fluid from which the "nuclein" had been precipitated, as has been stated, was now poured into four volumes of alcohol, and the precipitate thus thrown down was washed and dehydrated by the action of alcohol and ether. This preparation, which is necessarily very impure and especially rich in organic salts, will be described and referred to as "residual material."

Thus, by fractional precipitation with acetic acid, in the presence of inorganic salts, we have obtained three preparations. The nucleoproteid, which is doubtless the body which Hammarsten denominated Proteid-α, is almost insoluble in pure water, but may be dissolved by minute quantities of ammonia and caustic soda. The body which we have termed nuclein, to indicate our opinion of its relation to the first substance, is soluble in water with the greatest ease.

By the addition of a trace of copper sulphate to a solution of the nucleoproteid in caustic soda a fine pink colour is produced, but not a shade of violet makes its appearance until a comparatively large amount of copper solution has been added, a reaction which resembles closely "the biuret reaction" with the proteoses. The "nuclein" by similar treatment gives only the faintest pink colour, the violet shade being observed even when a very small amount of copper sulphate is used, while the "residual material" produces a violet colour from the beginning.

It has recently been shown by one of us* that the nucleoproteid of

* Walter Jones and G. H. Whipple, "The Nucleoproteid of the Suprarenal Gland," 'Amer. Jour. of Phys.,' vol. 7 (1902), p. 423.

the pancreas, prepared in substantially the same manner as the preparations employed in the present research, yields, when subjected to hydrolytic treatment, two of the xanthine bases, viz., guanine and adenine, and in a ratio which closely approximates four equivalents of the former to one of the latter. The "nuclein" and "residual material" of the present research were also found to yield xanthine bases on hydrolysis with sulphuric acid. All the three preparations under discussion contain phosphorus, all are completely precipitated from aqueous or faintly alkaline solutions by the addition of a trace of hydrochloric acid, and all yield precipitates when their neutral solutions are boiled.

Optical Properties of the Nuclein.

We had convinced ourselves by the following experiment that the specific rotation of the substance which we have denominated "nuclein" would be found to be greater than that of the nucleoproteid, before we had the opportunity of making a careful optical examination of the former substance.

A perfectly neutral solution of the neucleoproteid was prepared by treating some of the substance with water and an insufficient amount of ammonia to effect complete solution. The filtered fluid, examined with the polarimeter in a 200 mm. tube, gave a rotation of 1° 46′. The solution was heated to boiling, and the coagulated albumin filtered off. The filtrate polarised in a 200 mm. tube gave a rotation of 1° 49′. Now, as is well known, the process of boiling, removing a portion of the albuminous matter previously forming part of the complex nucleoproteid molecule, converts the latter into a nuclein. As the length of the tube was the same, and the angle of rotation remained sensibly constant in our experiment, a decrease in the amount of matter in solution (equal to the coagulated albumin removed from it) must mean an increase in the specific rotation.

The following direct determination of the specific rotation of the nuclein was made. The body was dissolved in water, and as the fluid was somewhat coloured, it was examined in a shorter tube than those which we have usually employed:—

Weight of substance.....	1·009 gramme.
Volume of solution	50 c.c.
Observed angle.....	+1° 18′
Length of tube	100 mm.

$$[\alpha]_D = +64°{\cdot}4.$$

The solution was treated with hydrochloric acid to precipitate the nuclein, and the filtered fluid examined in a tube 200 mm. long. The rotation was slightly negative (0°9′). In reference to this observa-

tion, we have to remark that we noticed several times that very slight lævorotatory filtrates were obtained when hydrochloric acid was used for precipitating the proteid, and especially when the acid fluid was allowed to remain in contact with the precipitate. Presumably, the negative rotation is due to an optically negative acid albumin being formed, which is soluble in the dilute hydrochloric acid.

As in the case of the nucleoproteid, a solution of the nuclein yields a coagulum on heating, and the rotation of the solution is not appreciably changed. This would lead one to assume the existence of a nuclein of which the specific rotation is greater than $+64°{\cdot}4$. It can easily be proved that such a substance exists in the preparation which we have designated "residual material."

A weighed amount of this substance was dissolved in a measured volume of water. The solution was examined with the polarimeter, treated with hydrochloric acid, and the amount of matter determined in the filtrate, which was found to be optically inactive. The following data were obtained:—

Weight of substance taken	0·520	gramme.
Weight of optically inactive matter ...	0·269	„
Weight of optically active matter	0·251	„
Volume of solution	25	c.c.
Observed angle.....	+1° 38′	
Length of tube.....	200	mm.

$$[\alpha]_D = +81°{\cdot}1.$$

C. Hammarsten's Preparation.

As we have already explained, Hammarsten's so-called nucleoproteid-β, which is obtained from an extract of pancreas made by boiling the finely comminuted gland in water must, *ipso facto*, be a nuclein. The results which we had obtained and which have been described, made it highly desirable that we should make an optical examination of this substance also. By slight departures* from the method described by Hammarsten, which were absolutely necessary to remove the colouring matter, but which cannot possibly have exercised any influence on the chemical nature of the product, we were able to prepare a nuclein which must have been identical with Hammarsten's preparation (nucleoproteid-β). The substance which we obtained is soluble in water, and gives a violet biuret reaction. Its solution was comparatively highly coloured, but possessed so great a rotatory

* We used ammonia for redissolving the nuclein, instead of a fixed alkali employed by Hammarsten. We also finally poured an aqueous solution of the nuclein into 95 per cent. alcohol, and washed by decantation with absolute alcohol and ether.

power that fairly satisfactory polarimetric observations could be made in solutions of great dilution. The substance is dextrorotatory. The following data were obtained :—

Weight of substance	0·200 gramme.
Volume of solution	25 c.c.
Observed angle (mean of eight readings) ...	0° 47′
Length of tube.....	100 mm.

$$[\alpha]_D = +97^\circ{\cdot}9.$$

On the Nucleohiston of the Thymus Gland.

It would seem quite easy to obtain this substance in any desired quantity by following the very simple method which Lilienfeld described in twenty lines.*

This method leads, however, to a product whose solutions are highly opalescent, and an optical examination could not be thought of. The cloudiness is so persistent, that for a long time we were inclined to believe it to be a property inherent in the substance. We finally succeeded, however, in obtaining solutions almost as colourless and transparent as distilled water. It is only necessary to extract Lilienfeld's preparation with a 5 per cent. solution of ammonium acetate and filter. The fluid filters very slowly, but perfectly clear and continuously. The solution was poured into 95 per cent. alcohol, and the precipitated proteid washed and dried with alcohol and ether, as described in connection with other preparations mentioned in this paper. The substance thus obtained was submitted to polarimetric examination, the solution being made with the aid of very dilute ammonia. The following data were obtained :—

Weight of substance	2·023 gramme
Volume of solution	50 c.c.
Observed angle	+3° 20′
Length of tube.....	220 mm.

$$[\alpha]_D = +37^\circ{\cdot}5.$$

On the Nucleoproteid of the Suprarenal Gland.

In a research carried on conjointly with G. H. Whipple,† one of us lately described the nucleoproteid of the suprarenal gland, and showed that this body is a thymo-nucleoproteid. Ultimate analyses showed that the nucleoproteids of the suprarenal gland of the ox and the

* Leon Lilienfeld, "Zur Chemie der Leucocyten," 'Zeit. f. physiol. Chem.,' vol. 18 (1894), p. 473.

† Walter Jones and G. H. Whipple, *op. cit.*, p. 423 (Sept. 1902).

sheep are identical, and scarcely differ in chemical composition from the nucleoproteid of the pancreas prepared substantially by the same method as that which has served for the researches described in this paper.

The following table exhibits the results of the ultimate analyses of these bodies, and, for purposes of comparison, the analyses made by Hammarsten of his preparation is also given:—

	Nucleoproteid of suprarenal gland of sheep.	Nucleoproteid of suprarenal gland of ox.	Nucleoproteid of pancreas of pig.	Hammarsten's preparation.
C	46·22	46·81	45·83	43·62
H	6·10	6·38	6·26	5·45
P	4·70	4·72	5·05	4·48
N	17·92	17·85	17·42	17·39

As closely as the analytical processes at command could determine, the nucleoproteids of the pancreas and the suprarenal gland yield guanine and adenine in the same relative proportions, and these appear to indicate that one molecule of a nucleinic acid, or of a nucleoproteid, may yield two different xanthine bases.

We must refer the reader to the paper quoted above for a description of a method of separating the nucleoproteid of the suprarenal gland. As is well known, the characteristic physiologically active constituent of this gland forms a dark brown pigment when exposed in aqueous solution to the oxidising action of the air. Aqueous extracts of the gland are therefore always highly coloured, and this colouring matter places great difficulties in the way of the preparation of substances from the gland which are intended for optical examination. While, therefore, the work on the nucleoproteid of the suprarenal gland is not as satisfactory as we could desire, it can nevertheless be stated most positively that this nucleoproteid also is dextrorotatory.

The method of isolation which we employed does not differ essentially from that employed in the research already referred to, except that the gland was extracted several times with acetic acid before removing the nucleoproteid. A substance was finally obtained which is too highly coloured for accurate polarimetric determinations, but which, even in the necessarily high dilutions which could alone be used, could easily be shown to be dextrorotatory.

The following data were obtained:—

Weight of substance	0·199 gramme.
Volume of solution	25 c.c.
Observed angle	+0° 23′
Length of tube	100 mm

$$[\alpha]_D = +48^\circ{\cdot}1.$$

The value of this rotation is liable to revision, but its direction is beyond question.

Before formulating the general conclusions which, it appears to us, may legitimately be deduced from the researches of which an account has been given in this paper, we may sum up our work in the following manner:—

Summary.

We have, in this paper, described six substances obtained from various glands and have given methods by which several of these may be isolated and obtained sufficiently free from colouring matters to admit of exact polarimetric determinations.

All six of these substances yield on hydrolysis, albuminous bodies, phosphoric acid, and purin derivatives, and all contain iron in stable combination; they are, therefore, all nucleoproteids in the wide sense of the term.

The methods of preparation were such as to exclude all dextrorotatory substances which are not of a proteid nature, and all preparations were shown to be free from substances which reduce Fehling's solution even on prolonged boiling. Nevertheless, all these substances were found to be dextrorotatory, having specific rotations for light of the wave-length of D which vary from 37°·58, that of the nucleohiston of the thymus gland, to 97°·9 that of Hammarsten's nuclein obtained from the pancreas and described by him as proteid.

General Conclusions.

1. The nucleoproteids (employing this term in its wider sense, as including the compounds of the nucleinic acids with albuminous substances) which are contained in the pancreas, the thymus, and the suprarenal gland are dextrorotatory albuminous compounds.

2. When a nucleoproteid, by the splitting-off of albuminous molecules, which in its original condition formed part of its more complex molecule, becomes converted into a nucleoproteid of the "nuclein" type, its specific rotation increases.

3. It is legitimate to infer that not only the well characterised and typical nucleoproteids which we have subjected to examination, but all the nucleoproteids, including in this term the so-called nucleins, form a class of dextrorotatory albuminous substances.

Whilst the facts which have come under our notice appeared to us so full of interest that it would not have been wise to defer their publication, we are perfectly alive to the importance of answering with the least possible delay a number of most interesting questions suggested by them. We are already actively engaged in the investigation of these questions and hope shortly to publish the results of our

enquiries. We trust, therefore, that during the next few months we may be permitted to work out so far as we are able, the problems which have been suggested by the new facts recorded in this paper.

Supplementary Bibliographical Note.

Since the above paper has been in print, it has come under the notice of one of us that the late Professor Alexander Schmidt, of Dorpat, in his published researches on the coagulation of the blood,* drew attention to the fact that among the soluble constituents of protoplasm was a body to which he gave the name of "Cytoglobin," and which he found to be dextrogyrous. So far as we are aware, this observation of A. Schmidt has never been noticed or quoted, either by systematic writers on physiological chemistry or by those who have devoted their attention to, or written upon, the subject which formed the life-work of the Dorpat professor. There can be no question that Schmidt's cytoglobin was an exceedingly impure mixture of nucleoproteids, an opinion which is based upon the fact that his substance contained 12·52 per cent. of ash, and that, on ultimate organic analysis, the amount of carbon found was 56·36 per cent., as compared with 45·83, the percentage of carbon in the nucleoproteid of the pancreas. Still the fact remains that this indefatigable worker, whose suggestive writings have been too little read, left data which prove that the so-called "Cytoglobin" was nucleoproteid in nature, though in no sense a definite proximate principle, and that this impure mixture of nucleoproteids was characteristically dextrogyrous.

March 4, 1903. A. G.

* Alexander Schmidt, 'Zur Blutlehre,' Leipzig, Verlag v. F. C. W. Vogel, 1892. Refer to the chapter entitled "Ueber den in Wasser löslichen Bestandtheil des Protoplasmas," &c. (pp. 127—142); 'Weitere Beitraege zur Blutlehre' (nach des Verfassers Tode herausgegeben). Wiesbaden, J. F. Bergmann, 1895. Refer to the chapter entitled "Zur Kenntniss des Protoplasmas und seiner Derivate" (pp. 201—249).

"A Note on a Form of Magnetic Detector for Hertzian Waves, adapted for Quantitative Work." By Dr. J. A. FLEMING, F.R.S., Professor of Electrical Engineering in University College, London. Received February 11,—Read March 5, 1903.

The known power of electrical oscillations to demagnetise iron or steel was first applied in the construction of a detector of Hertzian waves, as far as the author is aware, by Mr. E. Rutherford.* The power possessed by electrical oscillations to annul the magnetic hysteresis of iron was discovered by Mr. G. Marconi and applied by him in the construction of his ingenious and extraordinarily sensitive Hertzian wave detector, for use in connection with wireless telegraphy.†

The following note describes a form of magnetic Hertzian wave detector, which has been constructed by the writer for the purpose of quantitative experiments in connection with Hertzian waves.

Every one who has experimented with a Hertzian oscillator, or electric wave radiator in any form, involving a spark gap, is well aware of the immense difference in the radiative power produced by slight alterations in the nature of the spark or the spark balls, and has felt the want of some instrument which shall indicate and measure exactly the intensity of the radiation. As a receiving instrument, the coherer or sensitive imperfect contact is of very little use quantitatively, because its indications are influenced by very slight accidental changes at the contact or contacts. Thus, the sensitiveness of the metallic filings coherer depends upon the manner in which it was left after its last use, and by the mode in which it is tapped or shaken, and the change in the conductivity which it experiences on the impact of an electric wave, is variable and uncertain. Hence, although sensitive as a mere wave detector, the coherer is of little or no use in quantitative work. On the other hand, the magnetic detector is not only superior to the coherer in sensitiveness when properly constructed, but is capable of being used as a measuring instrument. In the form in which it was constructed by Mr. Rutherford, an extremely fine bundle of iron or steel wires was magnetised by means of a magnet, or by being placed in the interior of a solenoid, and then demagnetised by an electrical oscillation passing through another coil

* See Mr. E. Rutherford, "On a Magnetic Detector of Electric Waves and some of its applications," 'Roy. Soc. Proc.,' 1896, vol. 60, p. 184; see also 'Phil. Trans.,' A, 1897, vol. 189, p. 1.

† Mr. G. Marconi, "Note on a Magnetic Detector for Electric Waves which can be employed as a Receiver for Space Telegraphy," 'Roy. Soc. Proc.,' 1902, vol. 70, p. 341.

surrounding it. The amount of demagnetisation was detected by means of a magnetometer. In this form, it has been much used in experimental work, but it was not a telegraphic receiver.*

In the sensitive telegraph receiver invented by Mr. Marconi the change in magnetisation of the iron, due to the temporary abolition of hysteresis, is detected by the production of a sound in a telephone connected to a secondary coil surrounding the iron.

After trying various forms, the writer has found that a convenient magnetic detector for Hertzian waves can be constructed in the following manner :—

On a pasteboard tube, about $\frac{3}{4}$ of an inch in diameter and 5 or 6 inches long, are placed six bobbins of hard fibre, each of which contains about 6000 turns of No. 40 silk covered copper wire. These bobbins are joined in series, and form a well-insulated secondary coil, having a resistance of about 6000 ohms. In the interior of this tube are placed seven or eight small bundles of iron wire, each about 6 inches in length, each bundle being composed of eight wires, No. 26 S.W.G. in size, previously well paraffined or painted with shellac varnish. Each little bundle of iron is wound over uniformly with a magnetising coil formed of No. 36 silk-covered copper wire in one layer, and over this, but separated from it by one or two layers of gutta-percha tissue, is wound a single layer of No. 26 wire, forming a demagnetising coil. This last coil is in turn covered over with one or two layers of gutta-percha tissue.

The magnetising or inner coils are connected in series with one another, so that when a current passes through the whole of them, it magnetises the whole of the wires in such a manner that contiguous ends have the same polarity. The outer or demagnetising coils are joined in parallel. Associated with this induction coil is a rotating commutator, consisting of a number of hard fibre discs secured on a steel shaft, which is rotated by an electric motor about 500 times a minute. There are four of these fibre discs, and each disc has let in its periphery a strip of brass, occupying a certain angle of the circumference. These wheels may be distinguished as Nos. 1, 2, 3, and 4. The brass sector of No. 1 occupies 95° of its circumference; the brass sectors of Nos. 2 and 3 occupy 135° of their circumference; and that of No. 4 disc 140° of its circumference. Four little springy brass brushes make contact with the circumference of these wheels, and therefore serve to interrupt or make electric circuits as the disc revolves. The function of the disc No. 1 is to make and break the

* Note added March 7th. A general term seems to be required to include all forms of wave-detecting devices. The author suggests the word *kumascope* (from κῦμα, a wave) for this purpose. Uncouth phrases, such as *anticoherer* or *self-decohering-coherer*, which have crept into use in connection with Hertzian wave telegraphy, would be rendered unnecessary.

circuit of the magnetising coils placed round the iron bundles, and thus by applying a magnetising current to magnetise them during a portion of one period of rotation of the disc, and leave them magnetised in virtue of magnetic retentivity during the remaining portion. The function of discs 2 and 3 is to short-circuit the terminals of the secondary coil of the bobbin during the time that the magnetising current is being applied by disc No. 1. A sensitive movable coil galvanometer is employed in connection with the secondary coil, one terminal of the galvanometer being permanently connected to one terminal of the secondary coil, and the other terminal connected through the intermittent contact made by the disc No. 4. This disc No. 4 is so set that during the time that the secondary coil is short-circuited, and whilst the battery current is being applied to magnetise the iron wire bundles, the galvanometer circuit is interrupted by the contact on disc No. 4.

The operations which go on during one complete revolution of the discs are as follows:—First the magnetising current of a battery of secondary cells is applied to magnetise the iron bundles, and during the time this magnetising current is being applied, the terminals of the fine wire secondary coil are short-circuited and the galvanometer is disconnected. Shortly after the magnetising current is interrupted the secondary bobbin is unshort-circuited, and an instant afterwards the galvanometer circuit is completed and remains completed during the remainder of one revolution. Hence, during a large part of one revolution, the iron wire bundles are left magnetised, but the magnetising current is stopped and the galvanometer is connected to the secondary coil. If during this period an electrical oscillation is passed through the demagnetising coils, an electromotive force is induced in the secondary bobbin by the demagnetisation of the iron and causes a deflection of the galvanometer coil. Since the interruptor discs are rotating very rapidly, if the electrical oscillation continues, these intermittent electromotive impulses produce the effect of a continuous current in the galvanometer circuit, resulting in a steady deflection, which is proportional to the demagnetising force being applied to the iron, other things remaining equal. If the oscillation lasts only a very short time, the galvanometer will make a small deflection; but if the oscillation lasts for a longer time, then the galvanometer deflection is larger, and tends to become steady.

By means of such an arrangement it is possible to verify the law according to which variation falls off with distance. The instrument can be employed also as a telegraphic receiving instrument, but its chief use will be for comparing together the wave-making power of different radiators. For this purpose the oscillation coils must be connected to two long connecting wires, or one end may be connected to the earth and the other to a vertical aerial. This detector serves, for

instance, to show in a very marked manner the great effect of slight differences in the surface of the spark balls. If a steady series of sparks from an induction coil is passed between the spark balls of a Hertz linear radiator, it will produce a steady deflection on a galvanometer connected with the above-described receiver placed at a distance. If the balls are then polished, the galvanometer deflection immediately increases considerably. If, on the other hand, the balls are slightly smeared with oil, the galvanometer deflection decreases. If the radiator is approached to the receiver, or withdrawn from it, corresponding variations in the galvanometer deflection take place.

Such an instrument will probably be found of great use in connection with the design of radiators and transmitters for Hertzian wave wireless telegraphy. Up to the present it has been generally difficult to ascertain whether an improvement in the signalling is due to an accidental increase in sensitiveness in the coherer, or to any alteration or change made in the transmitter.

Similarly, the instrument promises to be of considerable use in the investigation of the transparency or opacity of various substances to Hertzian waves, not merely qualitatively, but in the determination of a coefficient of absorption. Preliminary experiments of this description made with the above-described instrument seem to promise for it a field of practical utility, both in the laboratory and in connection with Hertzian wave telegraphy.

In the numerous experiments which finally resulted in the construction of the above-described form of wave detector, it was found to be essential to have the iron core in the form of a number of small bundles of iron wire, each wound over with its own magnetising and demagnetising coil. No good results could be obtained when the iron core was in the form of a large bundle, say half an inch in diameter, and enveloped by a single magnetising and demagnetising coil.

Another condition of success is the short-circuiting of the fine wire secondary coil during the time of magnetisation of the core. The core can be indefinitely increased in size, provided the augmentation of mass is obtained by multiplying small individual cores, each consisting of not more than eight or ten fine iron wires, and each wound over with a separate magnetising and demagnetising coil. The electromotive force in the secondary coil can in this manner be increased as much as is desired, and a very sensitive wave detector produced. The commutator can be driven either by an electric motor or by any other source of power.

In conclusion, I have pleasure in mentioning the intelligent assistance rendered to me by Mr. A. Blok in the experiments conducted in connection with this appliance.

"A New Form of Self-restoring Coherer." By Sir OLIVER LODGE, F.R.S. Communicated verbally March 5,—Received in Manuscript March 18, 1903.

On the general subject of the detection of Hertzian waves the writer took the opportunity of the discussion on Dr. Fleming's paper (p. 398), to describe briefly the latest form of coherer, which Dr. Muirhead and he had brought out and always now employed in connection with space telegraphy, and which their assistant Mr. E. E. Robinson had helped to work out. It might be described as a development of the mercury form of coherer described some years ago by Lord Rayleigh, and again in a modified fashion by Mr. Rollo Appleyard. In Lord Rayleigh's form this consisted of a pool of mercury cut across with a paraffined knife, and the two half pools connected to a battery and key. As soon as the key was depressed so as to throw a few volts on to the intervening film of oil, the electrostatic pressure seemed to squeeze the oil out, and the pools of mercury became one.

Needle points dipping in oil and mercury were tried as practical coherers, the points being pulled out electromagnetically every time a signal arrived. Rotating forms of contact for automatic decoherence were also tried in various forms, and ultimately the method took the form of a rotating sharp-edged steel wheel, about half an inch in diameter, constantly touching a pool or column of mercury on which was a thin layer of oil. No effective contact occurs between the wheel and the mercury, notwithstanding the immersion, because of the film of oil; but the slightest difference of potential applied to the two, even less than one volt, is sufficient to break the film down and complete a circuit, which, however, the rotation of the wheel instantaneously breaks again. The spark is so sudden that for its purposes the wheel is for the instant virtually stationary, and yet the decohesion is so rapid that signals can be received in very rapid succession. The definiteness of the surfaces and of the intervening layer make the instrument remarkably trustworthy, and the thinness of the insulating film makes it very sensitive. In fact a single cell of a battery cannot be employed as a detector, because it is of too high a voltage for the film to stand. A fraction of a volt is employed by a potentiometer device—usually something like one-tenth of a volt—and it is adjusted to suit circumstances. The battery acts through the coherer direct on a low resistance recorder, and the record on the strip shows every character of the arriving pulses, and exhibits any defect in the signalling. Provided that every joint and contact, except the one intended to be filmed, is thoroughly good, the coherer in this form is so definite and satisfactory that it becomes safe to say that the only outstanding defects are those which occur at the sending end. The signals are

picked up and recorded precisely as they are emitted, as has been tested by intercalating a siphon recorder in a much diluted tapping circuit at the sending end, so as to get a record with which to make comparison. The traces obtained at the two ends are identical to a surprising degree.

The mercury level has an adjustment which is easily made. One precaution is to keep the rim of the wheel clear of dust, which is done by a cork or leather pad pressed lightly against it by a spring.

The instrument is not at all sensitive to tremor, and requires no particular delicacy of adjustment. The wheel has to be positive, the mercury negative.

A telephone in circuit, through a transformer or otherwise, affords an easy method of discriminating the signals by ear. The speed of the wheel gives another convenient adjustment to suit various circumstances.

"On Central American Earthquakes, particularly the Earthquake of 1838." By Admiral Sir JOHN DALRYMPLE HAY, Bart., G.C.B., F.R.S. Received March 6,—Read March 19, 1903.

The very interesting report of Dr. Tempest Anderson and Dr. J. S. Flett on "Recent Volcanic Eruptions in the West Indies," induces me to suggest that inquiries should be made in Colombia and in Central America as to disturbances in those regions, in order to trace the connection existing between the earthquakes and volcanic eruptions which are there so prevalent, and those in the West Indies.

Doubtless Mr. Welby, who has just returned to his post at Sta. Fé de Bogotá, might be able to obtain thence some information on this matter.

In the British Association Report, of 1858, upon Earthquakes, those of 1835 and of 1841 are given; but, unfortunately, no information is given as to the effect of those disturbances from any British source.

The British Association in both cases seem to have been mainly indebted for their information to the Académie, and hence the effects in Martinique and Guadeloupe are quoted, but nothing is reported from our West India Islands.

One of the most terrible disturbances in its effects, that of 1838, is not even alluded to. The only record of it is in the work of E. G. Squier, late Chargé d'Affaires from the U.S. of America to the Republics of Central America (2 vols., Appleton, New York, 1852), who was commissioned by his Government to report upon that region in reference to the Nicaraguan Canal. Vol. 1 has the map; vol. 2, pp. 114, 115, and p. 162 alludes to the eruptions of Cosequina in 1838,

and in describing the earthquakes and disturbances and eruptions of 1835, speaks of their effects in New Granada, Nicaragua, Popayan, Bogotá, Sta. Marta, Caracas, Hayti, Curaçao, and Jamaica.

I think it may be of interest to record my personal observations of the earthquakes and eruptions of 1838 as given in the log of H.M.S. "Imogene," Captain (afterwards Sir Henry) Bruce, in which I was then serving.

On Friday, November 16, 1838, noon, Lat. 6° 2′ N., Long., 82° 9′ W. Rio Lejo (to which we were bound from Panama) bearing N. 38, W. 490 miles. At 10 P.M. we felt the shock of an earthquake.

Tuesday, November 20, 1838, at 3.5 A.M., noon of that day, Lat., 9° 17′ N., Long. 83° 2′ W. Three heavy shocks in succession. Calm c. r. The topsails were lowered on the cap, and the ship lay till daylight, uncertain whether she had struck on a wreck or whether she was damaged. She rolled over to the limit of her capacity on three successive occasions.

At daylight (6 A.M.) all hands were sent for to see an extraordinary spectacle. The sea was white, as if it had been mixed with marl, and, as far as the eye could reach, the sea was covered with fish of various kinds stunned by the concussion. The boats picked up a sufficient supply for the ship's company, principally albacore, bonita, and dolphin. Some turtle were also captured.

We anchored in Rio Lejo on November 25, and remained there till November 30, 1838.

On the 1st December, we communicated with H.M.S. "Sulphur," Captain E. Belcher, then surveying the Gulf of Fonseca. Cosequina was in eruption, but El Viejo was quiescent. Belcher had ascended El Viejo on February 10, 1838, without difficulty, but Cosequina was in active eruption.

No record, so far as I know, has been published of this seismic disturbance; and although newspapers, received sometime after at Guaymas, reported synchronous disturbances at Tobago and elsewhere, no information except that contained in Mr. Squier's report is to be found on the subject.

It would seem desirable that the inquiry, so well carried out by Dr. Tempest Anderson and Dr. Flett should be extended into Colombia and Central America. The disturbance to which I have alluded above is traceable for more than 1500 miles.

The Colonial Office informs me that an earthquake was reported at St. Lucia on January 11, 1839, doing much damage. No information is recorded as to Tobago.

"The Emanations of Radium." By Sir WILLIAM CROOKES, F.R.S.
Received March 17,—Read March 19, 1903.

A solution of almost pure radium nitrate which had been used for spectrographic work, was evaporated to dryness in a dish, and the crystalline residue examined in a dark room. It was feebly luminous.

A screen of platinocyanide of barium brought near the residue glowed with a green light, the intensity varying with the distance separating them. The phosphorescence disappeared as soon as the screen was removed from the influence of the radium.

A screen of Sidot's hexagonal blende (zinc sulphide), said to be useful for detecting polonium radiations, was almost as luminous as the platinocyanide screen in presence of radium, but there was more residual phosphorescence, lasting from a few minutes to half an hour or more according to the strength and duration of the initial excitement.

The persistence of radio-activity on glass vessels which have contained radium is remarkable. Filters, beakers, and dishes used in the laboratory for operations with radium, after having been washed in the usual way, remain radio-active; a piece of blende screen held inside the beaker or other vessel immediately glowing with the presence of radium.

The blende screen is sensitive to mechanical shocks. A tap with the tip of a penknife will produce a sudden spark of light, and a scratch with the blade will show itself as an evanescent luminous line.

A diamond crystal brought near the radium nitrate glowed with a pale bluish-green light, as it would in a "Radiant Matter" tube under the influence of cathodic bombardment. On removing the diamond from the radium it ceased to glow, but, when laid on the sensitive screen, it produced phosphorescence beneath, which lasted some minutes.

During these manipulations the diamond accidentally touched the radium nitrate in the dish, and thus a few imperceptible grains of the radium salt got on to the zinc sulphide screen. The surface was immediately dotted about with brilliant specks of green light, some being a millimetre or more across, although the inducing particles were too small to be detected on the white screen when examined by daylight.

In a dark room, under a microscope with a $\frac{2}{3}$-inch objective, each luminous spot is seen to have a dull centre surrounded by a luminous halo extending for some distance around. The dark centre itself appears to shoot out light at intervals in different directions. Outside the halo, the dark surface of the screen scintillates with sparks

of light. No two flashes succeed one another on the same spot, but are scattered over the surface, coming and going instantaneously, no movement of translation being seen.

The scintillations are somewhat better seen with a pocket lens magnifying about 20 diameters. They are less visible on the barium platinocyanide than on the zinc sulphide screen.

A powerful electro-magnet has no apparent effect on the scintillations, which appear quite unaffected when the current is made or broken, the screen being close to the poles and arranged axially or equatorially.

A solid piece of radium nitrate is slowly brought near the screen. The general phosphorescence of the screen as visible to the naked eye varies according to the distance of the radium from it. On now examining the surface with the pocket lens, the radium being far off and the screen faintly luminous, the scintillating spots are sparsely scattered over the surface. On bringing the radium nearer the screen the scintillations become more numerous and brighter, until when close together the flashes follow each other so quickly that the surface looks like a turbulent luminous sea. When the scintillating points are few there is no residual phosphorescence to be seen, and the sparks succeeding each other appear like stars on a black sky. When, however, the bombardment exceeds a certain intensity, the residual phosphorescent glow spreads over the screen, without, however, interfering with the scintillations.

If the end of a platinum wire which has been dipped in a solution of radium nitrate and dried is brought near the screen, the scintillations become very numerous and energetic, and cease immediately the wire is removed. If, however, the end of the wire touches the screen, a luminous spot is produced, which then becomes a centre of activity, and the screen remains alive with scintillations in the neighbourhood of the spot for many weeks afterwards.

"Polonium" basic nitrate produces a similar effect on the screen, but the scintillations are not so numerous.

Microscopic glass, very thin aluminium foil, and thin mica do not stop the general luminosity of the screen from the X-rays, but arrest the scintillations.

I could detect no variation in the scintillations when a rapid blast of air was blown between the screen and the radium salt.

A beam of X-rays from an active tube was passed through a hole in a lead plate on to a blende screen. A luminous spot was produced on the screen, but I could detect no scintillations, only a smooth uniform phosphorescence. A piece of radium salt brought near gave the scintillations as usual, superposed on the fainter phosphorescence caused by the X-rays, and they were not interfered with in any degree by the presence of X-rays falling on the same spot.

During these experiments the fingers soon become soiled with radium,

and produce phosphorescence when brought near the screen. On turning the lens to the, apparently, uniformly lighted edge of the screen close to the finger, the scintillations are seen to be closer and more numerous; what to the naked eye appears like a uniform "milky way," under the lens is a multitude of stellar points, flashing over the whole surface. A clean finger does not show any effect, but a touch with a soiled finger is sufficient to confer on it the property. Washing the fingers stops their action.

It was of interest to see if rarefying the air would have any effect on the scintillations. A blende screen was fixed near a flat glass window in a vacuum tube, and a piece of radium salt was attached to an iron rocker, so that the movement of an outside magnet would either bring the radium opposite the screen or draw it away altogether. A microscope gave a good image of the surface of the screen, and in a dark room the scintillations were well seen. No particular difference was observed in a high vacuum; indeed, if anything, the sparks appeared a trifle brighter and sharper in air than in vacuo. A duplicate apparatus in air was put close to the one in the vacuum tube, so that the eye could pass rapidly from one to the other, and it was so adjusted that the scintillations were about equal when each was in air. The vacuum apparatus was now exhausted to a very high point, and the appearance on each screen was noticed. Here again I thought the sparks in the vacuum were not quite so bright as in air, and on breaking the capillary tube of the pump, and observing as the air entered, the same impression was left on my mind; but the differences, if any, are very minute, and are scarcely greater than might arise from errors of observation.

It is difficult to form an estimate of the number of flashes of light per second. But with the radium at about 5 cm. off the screen they are barely detectable, not being more than one or two per second. As the distance of the radium diminishes the flashes become more frequent, until at 1 or 2 cm. they are too numerous to count.

[*Added March* 18.—On bringing alternately a Sidot's blende screen and one of barium platinocyanide, face downwards, near a dish of "polonium" sub-nitrate, each became luminous, the blende screen being very little brighter of the two. On testing the two screens over a crucible containing dry radium nitrate, both glowed; in this case the blende screen being much the brighter. Examined with a lens, the light of the blende screen was seen to consist of a mass of scintillations, while that of the platinocyanide screen was a uniform glow, on which the scintillations were much less apparent.

The screens were now turned face upwards so that emanations from the active bodies would have to pass through the thickness of card before reaching the sensitive surface. Placed over the "polonium"

neither screen showed any light. Over the radium the platinocyanide screen showed a very luminous disc, corresponding with the opening of the crucible, but the blende disc remained quite dark.

It therefore appears that practically the whole of the luminosity on the blende screen, whether due to radium or "polonium," is occasioned by emanations which will not penetrate card. These are the emanations which cause the scintillations, and the reason why they are distinct on the blende and feeble on the platinocyanide screen, is that with the latter the sparks are seen on a luminous ground of general phosphorescence which renders the eye less able to see the scintillations.

Considering how coarse-grained the structure of matter must be to particles forming the emanations from radium, I cannot imagine that their relative penetrative powers depend on difference of size. I attribute the arrest of the scintillating particles to their electrical character, and to the ready way in which they are attracted by the coarser atoms or molecules of matter. I have shown that radium emanations cohere to almost everything with which they come into contact. Bismuth,* lead, platinum, thorium, uranium, elements of high atomic weight and density, possess this attraction in a high degree, and only lose the emanations very slowly, giving rise to what is known as "induced radio-activity." The emanations so absorbed from radium by bismuth, platinum, and probably other bodies, retain the property of producing scintillations on a blende screen, and are non-penetrating.]

It seems probable that in these phenomena we are actually witnessing the bombardment of the screen by the electrons† hurled off by radium with a velocity of the order of that of light; each scintillation rendering visible the impact of an electron on the screen. Although, at present, I have not been able to form even a rough approximation to the number of electrons hitting the screen in a given time, it is evident that this is not of an order of magnitude inconceivably great. Each electron is rendered apparent only by the enormous extent of lateral disturbance produced by its impact on the sensitive surface, just as individual drops of rain falling on a still pool are not seen as such, but by reason of the splash they make on impact, and the ripples and waves they produce in ever-widening circles.

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* I have been quite unable to detect any lines but those of bismuth (and of known impurities) in the spectrum of the strongest and most active "polonium" salt I have been able to procure.

† Radiant matter, satellites, corpuscles, nuclei; whatever they are, they act like material masses.

BAKERIAN LECTURE.—"On the Constitution of the Copper-Tin Series of Alloys." By C. T. HEYCOCK, F.R.S., and F. H. NEVILLE, F.R.S. Received February 26,—Lecture delivered February 26, 1903.

(Abstract.)

This paper is an attempt to fill a very serious gap in the study of alloys. As a rule, an alloy begins to be interesting when the temperature of the liquid alloy has fallen to its freezing point. This point, which records the moment when solid first appears in the liquid, is easily observed on account of the evolution of latent heat that occurs on the formation of solid, and if the freezing points of all the alloys of a series are determined, we can plot the freezing-point curve. Many such curves have been traced in the last ten years: that of the copper-tin alloys is given by the upper line in our diagram. The curve consists of several branches cutting each other in angular points. The one thing that these curves record without ambiguity is the number of different solids that can crystallise out of the liquid alloys, for each branch corresponds to the crystallisation of a different substance. But this is almost all that such curves tell us with certainty. They do not tell us whether the solids forming are the pure metals, or pure compounds, or crystalline solid solutions of the metals. Other experiments are needed to decide such questions.

The other great branch of the study of alloys consists in the microscopic examination of the solid alloys after they have cooled to ordinary temperature, that is to say, after they have, in general, ceased to undergo change. Between these two series of experiments there is an enormous gap of temperature, it may be 1000 or 500 degrees, and it is in this range of temperature that the whole life-history of the alloy, regarded as an organism, is to be found. The only fruitful experiments we know of dealing with this intermediate region are the cooling curves of Roberts-Austen and Stansfield. These observers traced automatically the whole of the cooling of the bronzes and obtained some remarkable results. They found that the evolution of heat at the freezing point was often succeeded at much lower temperatures by other evolutions of heat, and that many of these must have occurred after the alloy had wholly solidified. These thermal changes point to important chemical or physical changes, though they do not tell us what these changes are. They suggest, however, that the final patterns found by the microscope in the solid alloys are likely to be very complicated, as they may contain several records superposed the one on the other. We found these patterns very beautiful, but hopelessly complicated.

It occurred to us, and this is the method of the paper, that we could simplify the phenomena by a systematic chilling of the ingots at selected temperatures. A number of small ingots of the same alloy were placed in separate tubes in a bath of tin, together with a recording pyrometer, the temperature was raised above the freezing point of the alloy, and the whole very slowly cooled, the slow cooling being an essential feature of the experiment. Ingots were then extracted at selected temperatures and rapidly chilled by immersion in water. The microscopic examination of these chilled ingots showed that it was quite easy to distinguish the large crystals, that had formed during the slow cooling preceding the chill, from the matter that was liquid when the ingot was withdrawn from the furnace.

Successive chills of an alloy exhibit the solid growing in amount as the temperature falls, and finally show the ingot completely full of solid. We thus obtain, with very reasonable accuracy, the temperature of complete solidification of the alloy ; and by applying the method to alloys with different percentages of tin we have traced a new curve, the "solidus," or curve of complete solidification. The solidus of the bronzes is the second line of the diagram. It is a remarkable line, made up of sloping, horizontal and vertical branches. As in the freezing-point curve, each branch corresponds to the crystallisation of a different solid.

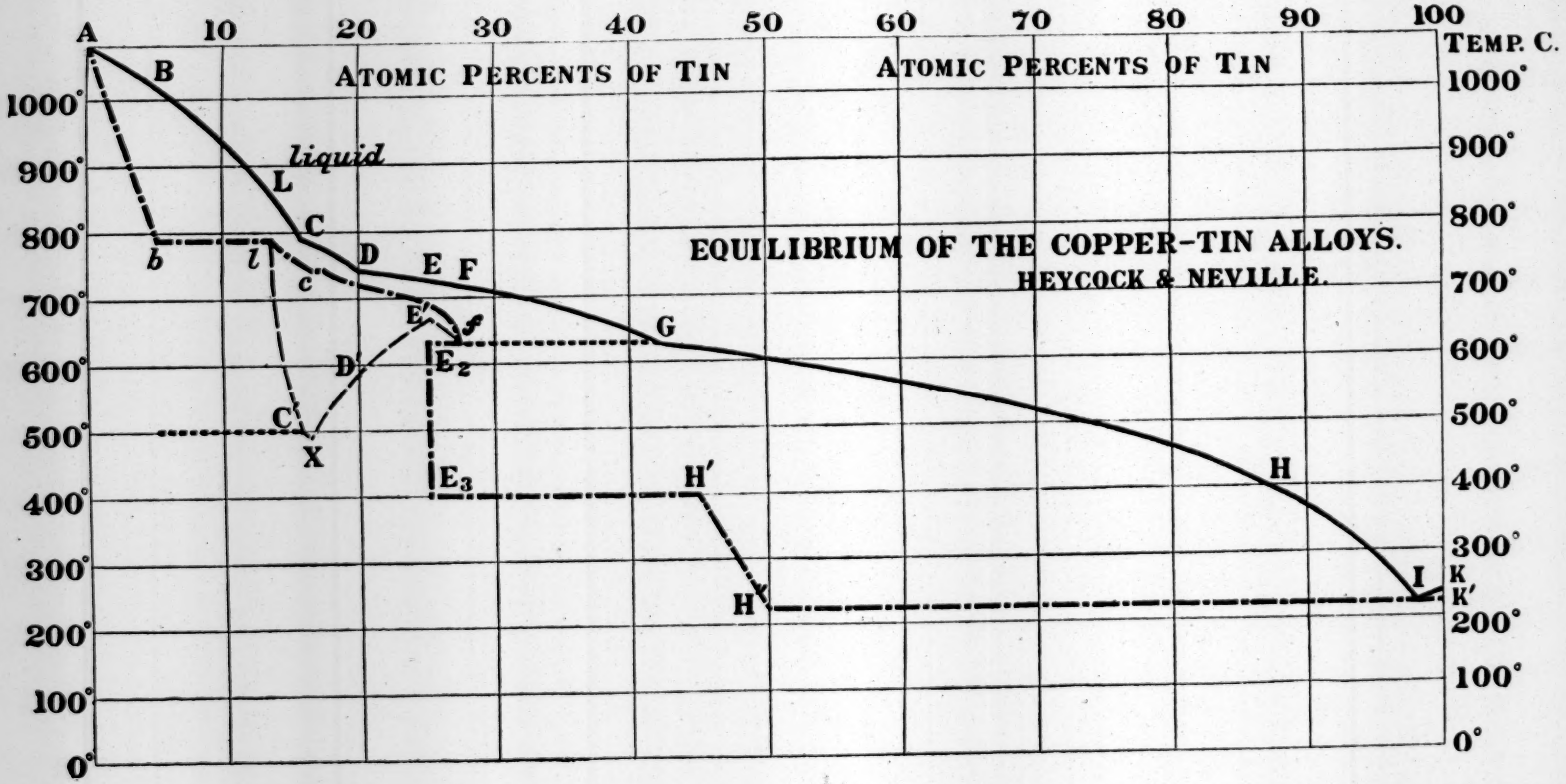
In the notation of Professor Roozeboom, the upper curve ABC...I, is called the liquidus, because all wholly liquid alloys lie above it; and the solidus, A*blcdef* $E_2E_3H'H''I$, is so named because all wholly solid alloys lie below it.

The solidus of the bronzes is remarkable for the very narrow range of temperature within which some alloys pass from the wholly liquid to the wholly solid state.

According to Roozeboom's theory, each sloping branch of the solidus, and there are four such in the diagram, corresponds to the crystallisation out of the liquid of a different series of solid solutions, each vertical part to the crystallisation of a pure body, and each horizontal part to the case of the solid alloy at temperatures immediately below the solidus, being a complex of two substances. Our examination of the chilled ingots has completely verified all these statements.

The evolutions of heat observed by Roberts-Austen and Stansfield at temperatures corresponding to the point C, D, G and H are due to definite chemical transformations in which one solid is decomposed and another is formed. Chills taken immediately above and below these critical temperatures reveal the nature of each change most clearly.

The transformations at C, D, and especially at H, are very slow and do not become complete unless the temperature is maintained constant for hours or days at a point slightly below the transformation temperature, but all these changes can be made to agree exactly with


EQUILIBRIUM OF THE COPPER-TIN ALLOYS.
HEYCOCK & NEVILLE.
ATOMIC PERCENTS OF TIN
ATOMIC PERCENTS OF TIN
TEMP. C.
liquid
A
B
L
C
D
E
F
G
H
I
K
K'
H'
b
l
c
C'
X
E2
E3
f
0
10
20
30
40
50
60
70
80
90
100
0°
100°
200°
300°
400°
500°
600°
700°
800°
900°
1000°

theory if time is allowed for them. The change at the G temperature is the breaking up of a solid solution into a mixture of the compound Cu_3Sn and liquid, and is instantaneous: here we have a case of a solid partially melting as it cools.

The curve lXE'*f* forms with the part of the solidus immediately above it an area, roughly triangular, within which all the alloys appear to be uniform solid solutions, but, as soon as an alloy cools to the curve, it becomes saturated and a new body crystallises out of the solid solution. One branch of the curve lXE'*f* corresponds to the crystallisation of a body rich in copper, the other to the crystallisation of a body rich in tin, which is probably the pure compound Cu_4Sn. The angle X (or rather C'), is the eutectic angle at which both bodies crystallise together, the whole phenomenon being exactly like crystallisation out of a liquid.

All the results obtained from the study of the chilled alloys are in harmony with the pyrometric work of Roberts-Austen and Stansfield, and many of the changes we have examined correspond to an evolution of heat recorded by them.

The paper is an extension of a short paper published by us in the 'Proceedings' of December, 1901.

"On the Formation of Barrier Reefs and of the Different Types of Atolls." By ALEXANDER AGASSIZ, For. Mem. R.S. Received February 7,—Read March 19, 1903.

The results here presented are based upon observations carried on during the past 25 years in Florida, the Bermudas, Bahamas, Cuba, Jamaica, and the West Indies in the Atlantic. They include in the Pacific the Galapagos, the Hawaiian Islands, the Great Barrier Reef of Australia, the Fiji Islands, and the Coral Reefs and Islands of the tropical Pacific, from the Marquesas to the Paumotus, the Society Islands, the Cook Archipelago, Niue, the Tonga, Ellice, Gilbert, and Marshall Islands, the Carolines and Southern Ladrones, and the Maldives, in the Indian Ocean.

Recognising that Darwin's theory did not explain the conditions observed, my reports were limited to descriptions of the different types of Coral reefs and of the causes to which they probably owed their formation, and no attempt was made to establish any independent general theory.

Beginning with the Barrier Reefs, we find that those of Fiji, the Hawaiian Islands, and the West Indies usually flank volcanic islands and are underlaid by volcanic rocks. Those of New Caledonia,

Australia, Florida, Honduras, and the Bahamas, are underlaid by outliers of the adjoining land masses, which crop out as islands and islets on the very outer edge of the Barrier Reefs. Some of the Barrier Reefs of the Society Islands, of Fiji, and of the Carolines, show that the wide and deep lagoons, separating them from the land mass, have been formed by erosion, from a broad fringing reef flat. Encircling reefs, such as characterise especially the Society Islands, hold to their central island or islands the same relation which a Barrier Reef holds to the adjoining land mass. Denudation and submarine erosion fully account for the formation of platforms upon which coral reef and other limestone organisms may build, either barrier or encircling reefs, or even atolls, rising upon a volcanic base, of which the central mass may have disappeared, as in Fiji, the Society and Caroline Islands.

We may next take the type of elevated islands of the Paumotus, the Fiji, the Gilbert, and the Ladrones, many composed only of tertiary limestones, others partly of limestone, and partly volcanic. We can follow the changes from an elevated island, like Niue, or Makatea in the Paumotus, to an island like Niau, through a stage like Rangiroa to that of the great majority of the atolls in the Paumotus. The reef-flats and outer reefs flanking elevated islands, hold peculiar relation to them, they are partly those of Barrier Reef and partly of Fringing Reef. We may also trace the passage of elevated plateaux like Tonga, Guam, and islands in Fiji, partly volcanic and partly limestone, to atolls where only a small islet or a larger island of either limestone or volcanic rock is left to indicate its origin. Atolls may also be formed upon the denuded rim of a volcanic crater, as at Totoya and Thombia in Fiji, as well as in some of the volcanoes east of Tonga.

In the Ellice and Marshall group and the Line Islands, are a number of atolls, the underlying base of which is not known, and where we can only follow the formation of the land rim of the atoll, as far as it is due to the agency of the trades or of the monsoons in constantly shifting the superficial material (prepared by boring organisms) which goes to form its rim. Many of the atolls in the Pacific are merely shallow sinks, formed by high sandbanks, thrown up around a central area.

Throughout the Pacific, the Indian Ocean, and the West Indies the most positive evidence exists of a moderate, recent elevation of the coral reefs. This is shown by the bosses, pinnacles, and undermined masses of modern or tertiary limestone left to attest it. The existence of honeycombed pinnacles of limestone within the lagoons of atolls, as shoals, islands, or islets, shows the extent of the solvent action of the sea upon land areas, having formerly a greater extension than at the present day. Signs of this solvent action are to be seen everywhere among coral reefs. Atmospheric denudation has played an

important part in reducing elevated limestone islands to the level of the sea, by riddling them with caverns and by forming extensive sinks, often taken to be elevated lagoons.

Closed atolls can hardly be said to exist; Niau in the Paumotus is the nearest approach to one, yet its shallow lagoon is fed by the sea through its porous ring. Sea water may pass freely into a lagoon at low tide over extensive shallow reef flats where there are no boat passages. The land area of an atoll is relatively small compared to that of the half-submerged reef flats. This is specially the case in the Marshall Islands and the Maldives where land areas are reduced to a minimum.

The Maldivian plateau with its thousands of small atolls, rings, or lagoon reefs, rising from a depth varying from 20 to 30 fathoms, is overwhelming testimony that atolls may rise from a plateau of suitable depth, wherever and however it may have been formed and whatever may be its geological structure. On the Yucatan plateau similar conditions exist regarding the formation of atolls, only on a most limited scale.

The great coral reef regions are within the limits of the trades and monsoons and areas of elevation, with the exception of the Ellice and Marshall Islands and some of the Line islands. The extent of the elevation is shown by the terraces of the elevated islands of the Paumotus, Fiji, Tonga, Ladrones, Gilbert, and West Indies, or by the lines of cliff caverns indicating levels of marine erosion.

In the regions I have examined the modern reef rock is of very moderate thickness, within the limits of depth at which reef builders begin to grow, and within which the land rims of atolls or of Barrier Reefs are affected by mechanical causes. This does not affect the existence of solitary deep-sea corals, of extensive growths of Oculina or Lophohelia at great depths, or in any way challenge the formation of thick beds of coralliferous limestone during periods of subsidence.

The Marquesas, Galapagos, and a few islands in the Society and West Indies have no corals, although they are within the limits of coral areas. Their absence is due to the steepness of their shores and to the absence or crumbling nature of their submarine platforms. Coral reefs also cannot grow off the steep cliff faces of elevated, coralliferous limestone islands.

Corals take their fullest development on the sea faces of reefs; they grow sparingly in lagoons where coralline algæ grow most luxuriantly. Nullipores and corallines form an important part of the reef-building material.

"The Electrical Conductivity imparted to a Vacuum by Hot Conductors." By O. W. RICHARDSON, B.A., B.Sc., Fellow of Trinity College, Cambridge. Communicated by Professor J. J. THOMSON, F.R.S. Received February 28,—Read March 26, 1903.

(Abstract.)

The experimental part of this paper is an investigation of the electrical conductivity of the space surrounding hot surfaces of platinum, carbon, and sodium, at low pressures. In addition, the first portion of the paper is occupied in deducing a theory by which the experimental results are explained. Some of the results that have been obtained with platinum were described in a preliminary note read before the Cambridge Philosophical Society on November 25, 1901.

The present paper is subdivided as follows :—

A. Theoretical investigation.
   1. Calculation of the saturation current.
   2. Equilibrium of corpuscles near a hot conducting plane of infinite area.

B. Experimental investigation.
   1. Experiments with platinum.
   2. Experiments with carbon.
   3. Experiments with sodium.

C. Conclusion.

The experiments show that the negative leak from hot wires at low pressures is a definite function of the temperature of the wire, and increases very rapidly as the temperature is raised. Professor McClelland* had previously found that this leak was independent of the pressure at pressures less than 0·04 mm., whilst Professor J. J. Thomson† had shown in addition that the current was carried by corpuscles or electrons.

The theory here put forward to explain these facts, and those to be described later, is based on the corpuscular theory of conduction in metals. On that view a metal contains a very great number of free corpuscles whose mean free path is comparable with that of a molecule in air at atmospheric pressure. The corpuscles must, therefore, be moving with a distribution of velocity given by the Boltzmann-Maxwell law. Since the corpuscles do not escape from the metal at ordinary temperatures, it is evident that there must be a discontinuity of potential at the surface which prevents their escape.

* 'Camb. Phil. Soc. Proc.,' vol. 10, p. 241, and vol. 11, p. 296.

† 'Phil. Mag.,' vol. 48, p. 547.

On raising the temperature of the metal, the energy of the corpuscles is increased, and at high enough temperatures some of them will be able to shoot through the surface into the surrounding space.

The number of corpuscles which escape at any temperature has been calculated on this view. The saturation current, which corresponds to the number emitted per second, is given by the equation

$$C = n\epsilon S\left(\frac{R\theta}{2m\pi}\right)^{\frac{1}{2}} e^{-\Phi/R\theta},$$

where

n is the number of free corpuscles in 1 c.c. of the metal,
ϵ the charge on an ion,
S the area of the hot-metal surface,
R the gas constant for a single corpuscle, whose mass is m,
θ the absolute temperature, and
Φ the work done by an ion in passing through the surface layer.

The rate at which energy is emitted, due to this cause, is also calculated.

Owing to the important part which the ionisation from hot bodies has played in certain recent cosmological theories, the equations which determine the equilibrium of corpuscles near a plane surface of hot metal of infinite extent are also given and solved.

The chief problem which has been attacked experimentally is the way in which the saturation current from a negatively-charged hot metal surface to a neighbouring electrode varies with the temperature of the metal.

In the case of platinum, the hot metal consisted of a fine wire spiral passing along the axis of a surrounding cylindrical electrode. The temperature was obtained from the resistance of the wire.

In the case of carbon, the leak was measured from a small lamp filament to a surrounding cylinder. The temperature was estimated in two ways: (1) by fastening a platinum and platino-iridium thermo-couple of very fine wire round the filament, and (2) from the resistance of the filament.

With sodium this method could not be adopted. The metal was distributed on the inner surface of a steel cylinder, and the current from it to an insulated wire inside the cylinder was measured. The temperature was obtained by a thermo-couple of copper and nickel. Owing, doubtless, to the peculiar shape of the electrodes and the somewhat high pressure of the gas, the current with sodium was never saturated. For this reason the current under a given voltage was measured instead of the saturation current.

Incidentally, it was found necessary to examine the relation between the current and the applied electromotive force. Current E.M.F. curves are given for all three substances, and, in the case of carbon,

for a considerable range of pressures. To account for the results at the higher pressures, it is necessary to assume that ions are produced by collisions.

The variation of current with temperature is examined over the following range :—

For platinum from 10^{-10} to 10^{-3} ampère per sq. cm. of surface.
For carbon „ 10^{-8} „ 2 „ „ „
For sodium „ 10^{-11} „ 2×10^{-2} ampère total current.

The corresponding ranges of temperature for platinum and sodium are roughly from 1000° C. to 1600° C., and from 100° C. to 450° C. respectively. The small currents from sodium were measured with a quadrant electrometer, but as a general rule, a sensitive D'Arsonval galvanometer with suitable shunts was used.

Perhaps the most striking result of the investigation is the relatively enormous currents which have been obtained. The biggest leak measured was 0·4 ampère from a carbon filament to an electrode placed near it; this corresponded to a current of 2 ampères per sq. cm. of the carbon surface, the potential on the filament being − 60 volts. The pressure in this experiment was only $\frac{1}{600}$th mm. This current and some of the largest currents from sodium were registered on a Weston ammeter.

In all these experiments the potentials employed were too small to maintain a discharge between the electrodes.

Throughout the range given above, the relation between the saturation current and the temperature was found to be represented by a formula of the type

$$C = A\theta^{\frac{1}{2}}e^{-b/\theta},$$

where A and b are constants for each conductor.

The values which have been found for these constants are—

For platinum, $A = 10^{26}$, $b = 4{\cdot}93 \times 10^4$.
For carbon, $A = 10^{34}$, $b = 7{\cdot}8 \times 10^4$. } $9{\cdot}7 \times 10^4$. } $11{\cdot}9 \times 10^4$. }
For sodium, $A = 10^{31}$, $b = 3{\cdot}16 \times 10^4$.

The value of A varies very rapidly with the value found for b, so that only its order of magnitude is given.

The number n of free corpuscles in a c.c. of the metal is calculated from A. For platinum this gives $n = 10^{21}$, whereas Professor Patterson* found 10^{22}. In the case of the other conductors, the number found is absurdly great compared with Patterson's values.

* 'Phil. Mag.' [6], vol. 3, p. 655.

The discrepancy can be made to disappear by assuming a small temperature variation of b. This assumption is shown to be consistent with the general nature of b.

The work required to drive an ion through the surface layer is calculated, in each case, from the value of b, to which it is proportional. Dividing by the charge on an ion this yields the discontinuity of potential at the surface of the conductor. The values found for this are: for sodium, $\delta\phi = 2{\cdot}45$ volts, for platinum $\delta\phi = 4{\cdot}1$ volts, and for carbon $\delta\phi = 6{\cdot}1$ volts. These numbers are inversely proportional to the cube roots of the respective atomic volumes. This leads to the conclusion that the work required to force a corpuscle out of a metal varies, approximately at any rate, inversely as the cube root of the atomic volume of the metal.

In all these experiments, the current when the hot wire is charged positively is small compared with that obtained with the metal negatively charged. Only in the case of sodium was the positive current large enough to deflect a sensitive galvanometer.

The results which have been obtained are shown to furnish a complete explanation of the phenomenon known as the Edison effect.

The fact that such enormous currents are obtained at such very low pressures confirms the conclusion that the ions are not produced from the gas by its interaction with the metal. Calculation shows that to obtain the currents registered with carbon, each gas molecule would have to give rise to 25 ions each time it collided with the hot metal surface.

The energy lost owing to the escape of the corpuscles is compared with the energy emitted in the form of ordinary electromagnetic radiation. The former is shown to be smaller than the latter at the temperatures at which measurements have been made, but it increases more rapidly with the temperature.

"On a New Series of Lines in the Spectrum of Magnesium." By A. FOWLER, A.R.C.Sc., F.R.A.S., Assistant Professor of Physics, Royal College of Science, South Kensington. Communicated by H. L. CALLENDAR, F.R.S. Received March 9,—Read March 26, 1903.

Although the spectrum of magnesium has been the subject of many investigations, certain lines which occur in the arc spectrum appear to have hitherto escaped notice. The lines in question are comparatively feeble, but on account of their theoretical interest it seems desirable to draw attention to them.

The new lines make their appearance in the spectrum when the arc is made to pass between poles consisting of magnesium rods, but they do not always appear with equal intensity. They are somewhat nebulous, especially on their less refrangible sides, so that their positions cannot be determined with great accuracy; but as nearly as they can be ascertained with the instruments at my disposal, the wave-lengths are (in air), 4511·4, 4251·0, 4106·8, and 4018·3.

A mere inspection of the photographs suggests that these lines constitute a regular series, associated with the much stronger series described by Rydberg*, having wave-lengths 5528·75, 4703·33, 4352·18, 4167·81, 4058·45, and 3987·08, according to the measures of Kayser and Runge. This view seems to be sufficiently confirmed by calculation.

Rydberg found that neither his own general formula nor that of Kayser and Runge could be applied with sufficient accuracy to the stronger series, and employed a combination of the two formulæ, namely,

$$n = a + \frac{b}{(m+\mu)^2} + \frac{c}{(m+\mu)^4},$$

where n is the oscillation frequency, m has the values 3, 4, 5, &c., and a, b, c, and μ are constants to be determined from four lines belonging to the series. For the magnesium series, the equation calculated by Rydberg is

$$n = 26{,}631{\cdot}44 - \frac{111{,}856{\cdot}92}{(m+0{\cdot}406)^2} + \frac{147{,}764{\cdot}05}{(m+0{\cdot}406)^4},$$

n being the oscillation frequency *in air*, and m having the values 3, 4, 5, 6, 7, 8 for the six lines named above.

Using the same formula for the new series, and calculating the constants from the four lines, the equation for frequencies *in vacuo* is

$$n = 26{,}595{\cdot}4 - \frac{102{,}496{\cdot}6}{(m+0{\cdot}618)^2} + \frac{168{,}840{\cdot}5}{(m+0{\cdot}618)^4}.$$

* 'Öfversigt af Kongl. Vet. Akad. Forhandl.,' 1893, Stockholm.

Another formula* which may be conveniently employed is

$$n = n_\infty - \frac{C}{(m+\mu)^2 - m_0}.$$

This formula gives for the two magnesium series the equations:

$$\text{"Rydberg" series, } n = 26{,}601{\cdot}49 - \frac{107{,}071{\cdot}37}{(m+1{\cdot}2304)^2 + 2{\cdot}13282},$$

$$\text{New series, } n = 26{,}587{\cdot}4 - \frac{100{,}033{\cdot}6}{(m+0{\cdot}495)^2 + 2{\cdot}38919},$$

n being the oscillation frequency *in vacuo* in each case.

It will be seen that the convergence frequency of the new series is as nearly equal to that of the Rydberg series as can be expected with the comparatively rough wave-lengths employed, and it may be added that in each case the constant m_0 is of unusual magnitude. These facts, in conjunction with the general characters and relative intensities of the lines, render it highly probable that the new series is associated with the Rydberg series as second and first subordinate series respectively.

Applying the formula to the calculation of the members of the new series for which $m = 3$ and $m = 2$, the corresponding wave-lengths in air are 5065·0 and 6674·5. The first is probably represented by a line having an approximate wave-length 5067, which is not so readily observed in the photographs as the others, because the plates employed are comparatively slow for this part of the spectrum, and if the exposure be lengthened, the banded spectrum of magnesium becomes strong enough to almost mask the line. The line 6674·5 is perhaps too far in the red to be conveniently observed, seeing that it is probably feeble and not well defined.

It may be reasonably concluded that the arc spectrum of magnesium includes two subordinate series of single lines in addition to the two well-known subordinate series of triplets. No such combination of series appears to have been previously noted in the spectrum of a metal, but two sets of series, each set consisting of a principal and two subordinate series, are well-known in the spectra of helium and oxygen.

The author desires to express his obligations to Mr. Herbert Shaw for valuable assistance in making the computations involved in investigating the various formulæ which have been suggested for series, as applied to the series which forms the subject of the present paper.

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* After much labour, this formula was arrived at by Mr. Herbert Shaw and the author as the one giving the most consistent results for series in general, but it was afterwards found that a similar formula, expressed in wave-lengths, had already been employed by Mr. Rummel ('Roy. Soc. Victoria Proc.,' vol. 10, Part I, 1897, p. 75).

"An Attempt to Estimate the Relative Amounts of Krypton and of Xenon in Atmospheric Air." By Sir WILLIAM RAMSAY, K.C.B., F.R.S. Received March 9,—Read March 26, 1903.

When Dr. Travers and I isolated krypton and xenon from air, we had very little idea of the total amount of liquid air from which, by its evaporation, these gases had been obtained. And we were then more concerned with the isolation of the gases in a pure state than in the determination of the proportion in which they exist in the atmosphere. Our knowledge of the composition of the air, however, is not complete until the total yield of krypton, xenon, neon, and helium has been determined. An estimation of the two last is being undertaken by Dr. Travers.

In our experiments on these gases* we did not measure the total quantity of air evaporated. We used liquid air for various purposes, and for some months we collected the dregs, allowing them to evaporate into a large gas-holder. We guessed (but it was only the roughest estimate) that we had accumulated in this manner the residues from about thirty litres of liquid air; and on this assumption we thought the following estimates not improbable:—Helium, 1—2 parts per million of gaseous air; neon, 1 per 100,000; krypton, 1 per million; xenon, 1 per 20,000,000. But they rested on a very insecure foundation of fact.

The first preliminary experiment was made to ascertain how much of the air which passes through the Hampson liquefier is converted into liquid. The results, however, were inaccurate, and I would rather cite the conclusions given by later experiments on a much larger scale. The compressor was run for several hours every morning and afternoon during six and a half days; the liquefied air was weighed after each run; and the escaping air passed through a large gas-meter, where its volume was registered. The air escaping had a somewhat lower density than ordinary air, owing to the partial removal of oxygen and argon; but the experiments were not sufficiently accurate to make it worth while to take this into account. The volumes given are, however, corrected for alterations of pressure and temperature. In all, 179·7 kilos. of gaseous air passed the meter, and 10·8 kilos. of liquid air were collected. During the collection, about 6 per cent. of the liquid air evaporates; adding this, 11·4 kilos. must have been the total weight of air liquefied. The total weight of air taken in by the compressor, therefore, and delivered to the liquefier was consequently 179·7 + 11·4 = 191·1 kilos, and the percentage liquefied a little under 6 per cent. The number may be taken without sensible error as 6 per cent. liquefaction.

* "Argon and its Companions," 'Phil. Trans.,' A, vol. 197, pp. 47—89.

During these runs the pressure was kept at 190 atmospheres; and the delivery of air through the escape valve of the liquefier remained fairly uniform. Separate readings, taken at different times, of the amount of air passing the meter gave 0·244, 0·230, 0·258, 0·261, 0·248, and 0·239 kilos. per minute; these figures testify to fairly uniform working.

It was of interest to see whether argon concentrated itself in the liquefied portion of the air, or whether most of it escaped as gas. The boiling points of the principal atmospheric gases are, on the absolute scale:—nitrogen, 77°·54; argon, 86°·90; oxygen, 90°·5; the differences are 9°·36 and 3°·6 respectively; it was therefore to be expected that the argon would concentrate in the liquid.

An experiment was therefore made in which 12·73 grammes of freshly collected liquid air was allowed to boil in a double-walled vacuum-tube, and the gases were led directly over metallic copper and magnesium lime; the resulting argon measured 165 c.c. at 0° and 760 mm. pressure. The weight of this argon was 0·2943 gramme, or from 1 gramme of liquid air, 0·02312 gramme. Now 1 gramme of atmospheric air contains 0·0129 gramme of argon; hence the process of liquefaction nearly doubles the content of argon in the air. It is, therefore, very advantageous to prepare argon from air which has been liquefied.

Acting on this suggestion, it appeared *à fortiori* probable that if the percentage of argon in air were doubled by liquefaction, the krypton and the xenon would be practically wholly removed and liquefied. And by submitting liquefied air to a second liquefaction, by boiling it off through the compressor, it appeared to hold that a concentration of the krypton and xenon would result, and that they would be found wholly in the liquefied portion. An experiment was therefore begun in which about 100 kilos. of gaseous air were passed through the liquefier; the liquefied portion, amounting to about 6 kilos., was again passed through the liquefier, somewhat added to by gaseous air, drawn in by the compressor. By an unfortunate accident, however, nine-tenths of the air collected during the second liquefaction was lost; and the amount of xenon and krypton in the remaining tenth, representing about 10 kilos. of gaseous air, did not appear to justify repetition of the troublesome experiment. I have no doubt, however, that had these experiments not had for their object the determination of quantity, but only the preparation of krypton and xenon, they would have effected the separation well.

The liquid air, resulting from the liquefaction of 6 per cent. of 191·1 kilos. of gaseous air, was sucked, several litres at a time, into a large glass balloon of about 5 litres capacity, fitted with an india-rubber cork, through which a wide tube passed, connected with a double-acting Fleuss pump, driven by an electric motor. Through

another hole in the cork there passed a siphon which could be closed by means of a brass stop-cock; this tube served to admit liquid air to the balloon. A manometer was also connected with the interior of the flask so as to register the pressure under which the liquid air was boiling. The air boiled at a pressure of about 250 mm., corresponding to a temperature of about $-195°$. The boiling was quite quiet, without bumping; it was sometimes necessary to warm the balloon gently in order to accelerate the evaporation. The object of distilling at a low temperature was to lower the vapour-pressure of the krypton and xenon in the liquid air, and so to lessen, or in great part to prevent their evaporation. The total liquid air was thus reduced to about 200 c.c.

The balloon containing this air was coupled with a large iron tube, holding about 20 kilos. of reduced copper heated to bright redness. The liquid air residue, naturally, consisted largely of oxygen, for the more volatile nitrogen had in great part evaporated. After passing over the copper the volume of gas was about 50 litres.

It may be contended that during the evaporation of the air, even at $-195°$, a large portion of the krypton and xenon may pass away as gas. It is not possible to estimate the amount lost in this manner; but at $-195°$, the vapour pressure of krypton is 2·8 mm., and that of xenon 0·02 mm. These figures have been arrived at in the following manner. Relying on the vapour pressures of mercury by Ramsay and Young, given in the *Trans. Chem. Soc.*, 1886, p. 50, and on the measurements made by Ramsay and Travers* of the vapour pressures of krypton and xenon, ratios were found between the absolute temperatures of mercury on the one hand, and of krypton and xenon respectively on the other, between the pressures 300 and 3000 mm., with two additional data—the temperatures of krypton corresponding to pressures of 9 and of 17·4 mm. They are as follows:—

Pressures.	Temp. of mercury. Degrees absolute.	Temp. of krypton. Degrees absolute.	Ratios.	Temp. of xenon. Degrees absolute.	Ratios.
mm.					
9·0	454·8°	94·2°	0·1851	—°	—
300	582·2	110·4	0·1897	148·9	0·2257
400	596·4	113·8	0·1909	153·2	0·2569
500	609·0	116·1	0·1907	156·8	0·2575
600	617·9	118·35	0·1915	159·7	0·2583
700	626·5	120·2	0·1918	162·0	0·2586
760	631·2	121·3	0·1922	163·9	0·2597
3000	726·8	142·2	0·1957	192·4	0·2647

* 'Phil. Trans.,' A, vol. 197, 1901, p. 72.

These ratios were mapped against the absolute temperatures of mercury, and were found, as usual, to give straight lines. The lines were extrapolated to lower temperatures, and the vapour pressures required were calculated from the extrapolated curves.

The portion of interest is given in the following table:—

Temp. C.	Vap. press. of krypton.	Vap. press. of xenon.
− 205°	0·27 mm.	0·0005 mm.
− 200	0·97 „	0·007 „
− 195	2·8 „	0·02 „
− 190	7·5 „	0·04 „
− 188·8	9·0 „	0·11 „
− 182·4	17·4 „	0·17 „

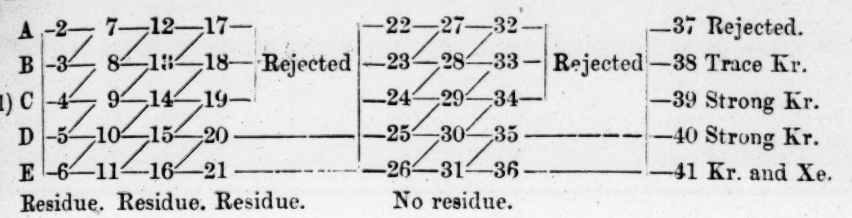
The last two data for krypton are the results of direct measurement. It may be mentioned here that the melting point of krypton is about − 169°, and that of xenon − 140°; and the boiling points at atmospheric pressure are, krypton − 151°·7, and xenon − 109°·1.

The nitrogen was removed from the 50 litres of gas by passing it over red-hot magnesium-lime mixture. The resulting crude argon measured 12·5 litres at 16° and 770 mm.; its weight is calculated from its known density as 21·3 grammes.

This argon was liquefied in a bulb immersed in liquid air, boiling under reduced pressure, so as to reduce the vapour pressure of the krypton and xenon; and the major part was re-transferred through a Töpler pump to the gas-holder from which it had passed to the liquefying bulb. About 1500 c.c. of the last portions to distil away were collected in five mercury gas-holders, each of a capacity of 300 c.c. The argon was now methodically fractionated according to the accompanying scheme.

The 1200 c.c., numbered (1) was, as described, distributed in five gas-holders. The contents of the first—the one first filled is termed A—were liquefied, and half the amount replaced in gas-holder A. The contents of B were liquefied, and A was filled, by allowing the liquid argon to evaporate under reduced pressure. The contents of C were liquefied along with what remained of B; and B was filled in like manner. D was liquefied, and C filled; and, finally, E, and D filled. The residue in the liquefying bulb, which evaporated very slowly after the argon had boiled away, was removed through the pump, and collected in a tube over mercury. The contents of A, B, C, D, and E are labelled (2), (3), (4), (5), and (6). The process was repeated and with this explanation the scheme can be understood.

Residue of 1500 c.c. :—



No. 41 was mixed with the residues, which contained much xenon, along with krypton. Nos. 38, 39, and 40 were mixed, and the fractionation continued in a small apparatus. The gases were now sparked with oxygen over soda, so as to remove traces of air; for the operations had now to be conducted with great care, and the spectra of the two samples showed traces of nitrogen. After withdrawal of oxygen from both sets of gas, by means of phosphorus, the krypton mixture was fractionated into three portions; 42, containing much argon; 43, rich in krypton; and 44, a non-volatile residue. The xenon residues, 41, were also solidified with liquid air, and placed for a few seconds in connection with the vacuum of the Töpler pump. The gas which was pumped off was added to 43, and the residue to 44. The bulb was now left for a quarter of an hour, so that equilibrium might be restored, and the stop-cock opened a second time. A bubble or two was removed with the pump. On making communication with the pump a third time, no gas escaped; and this is not remarkable, for the vapour pressure of xenon is little over 0·1 mm. at that temperature. It was therefore assumed that the final residue was pure xenon; and, indeed, its spectrum showed no trace of the krypton lines. And it may also be taken for granted that very little xenon was added to No. 43. The densities of Nos. 42 and 43 were determined; and from the known densities of argon and krypton, their relative proportions were calculated.

The density of 42 was found to be 21·31, corresponding to the percentage composition, argon, 93·5 per cent., krypton, 6·5 per cent.; the density of 43 was 39·43, implying a mixture of argon, 6·6 per cent., krypton, 93·4 per cent. The volume of No. 42, reduced to 0° and 760 was 22·0 c.c.; it therefore contained 1·43 c.c. of krypton; that of No. 43 was 6·5 c.c., and it contained 6·1 c.c. of krypton. The total volume of krypton was therefore 7·5 c.c., and its weight 0·0028 gramme; the volume of the xenon, reduced to 0° and 760 was 0·87 c.c., and its calculated weight, 0·0005 gramme.

These results are reproduced in the following tabular statement :—

Air passed through liquefier.....	191·1 kilogrammes.	
Air liquefied	11·3 „	= 5·91 p.c.
Argon obtained (11·8 litres at 0° & 760)	21·3 grammes.	
„ per cent. of gaseous air.....	0·0118; of liquid air, 0·1885.	

Total krypton obtained.....	0·0028 gramme.
„ xenon obtained	0·0005 „
Percentage krypton in gaseous air	0·000014 by weight.
„ xenon „ „	0·0000026 „

Krypton equal to 1 part by weight in about 7 millions of air; by volume, 1 part in 20 millions.

Xenon equal to 1 part by weight in about 40 millions of air; by volume, 1 part in 170 millions.

As before remarked, it is not maintained that all the krypton and all the xenon have been separated; it is likely, however, that the separation of the xenon was more perfect than that of the krypton. The results are merely brought forward as the result of a careful experiment to quantitatively isolate these gases.

I have to express my cordial thanks to Mr. E. C. C. Baly and to Mr. Inglis for aid in carrying out part of these operations.

As a quantity of pure krypton, sufficient for determination of density, had been collected, occasion was taken to redetermine the density of that gas. It was submitted to careful fractionation; a considerable portion was rejected as possibly containing argon, and the dregs were set aside as possibly having contained xenon. The substance weighed had a low vapour pressure,—about 15 mm. at the temperature of the liquid air used in fractionating. The separation of the lighter and heavier portions was repeated four times, the density having been determined on each occasion, with only small differences. Finally, a very careful determination of density was carried out, with the following results:—

Volume of density-bulb	7·268 c.c.
Temperature	15°·57 C.
Pressure on gas, corrected	754·0 mm.
Weight.....	0·02488 gramme.
Hence density, compared with O = 16...	40·81.

Previous determinations with two samples of gas, one fractionated from argon, the other fractionated from xenon, gave 40·82 and 40·73 respectively as the density. The result given above is in perfect concordance with these figures. The chief cause of error is in the weight; I think it would be fair to regard two units in the fifth place as the limit of error, which gives a possible divergence of about 1 part in 1200.

The atomic weight of krypton would accordingly be 81·62; the mean of former determinations is 81·28. This is in accordance with its position in the periodic table, which lies between bromine, 80, and rubidium, 85.

"Some Physical Properties of Nickel Carbonyl." By JAMES DEWAR, M.A., Sc.D., LL.D., F.R.S., Jacksonian Professor in the University of Cambridge, and HUMPHREY OWEN JONES, M.A., Fellow of Clare College, Jacksonian Demonstrator in the University of Cambridge. Received March 3,—Read March 26, 1903.

The properties of nickel carbonyl have until recently been the subject of but few investigations. Dr. Mond and his collaborators in the discovery of this remarkable substance determined some of its physical properties, including its boiling point, specific gravity, and vapour density. Subsequently Dr. Mond, in association with Professor Nasini, made observations on its molecular refraction and thermal expansion.

A substance of the peculiar molecular structure of nickel carbonyl seemed to call for further study. The investigation described in the present paper was carried out in the winter of 1901; the authors' intention being to make a complete study of the stability of the compound both in the gaseous and liquid conditions. While the work was in progress a paper by Mittasch was published* containing an account of an admirable and exhaustive investigation of the velocity of the reaction between nickel and carbon monoxide, including the heat of formation and vapour tensions of the compound, covering part of the ground which we had examined. A number of interesting problems examined in the course of our inquiry however remain, which have not been touched upon by previous investigators, and to some of these the present paper is devoted.

The vapour density of nickel carbonyl was determined by Mond, Langer and Quincke† in air, at 50° C. by Victor Meyer's method. The value obtained was 86·7, while theory requires 85. The vapour density at this temperature is quite normal, and there is no evidence of association even at this temperature only some 7° C. higher than the boiling point of the compound. It was found that the vapour exploded at 60° C. with a flash of light, and carbon dioxide was detected among the products of decomposition. Berthelot‡ explained the explosion as being due to the production of carbon dioxide by the intermolecular reaction:—

$2CO = CO_2 + C$, which was observed to take place when carbon monoxide acts on nickel at 350°—450° C. Later§ it was found that the action only proceeded in this way to a small extent, when

* 'Zeit. Phys. Chem.,' 1902, vol. 40, p. 1.
† 'Jour. Chem. Soc.,' 1890, vol. 57, p. 749.
‡ 'Compt. Rend.,' vol. 112, p. 1343.
§ 'Ann. Chim. Phys.,' 1901 [7], vol. 22, p. 304.

nickel carbonyl vapour was decomposed. The reaction $2CO = CO_2 + C$ liberates 38·8 kilogramme calories, so that if the heat of formation of nickel carbonyl is less than 77·6 kilogramme calories per molecule, this explanation would be valid. This condition is satisfied since Mittasch* has shown that the heat of formation of nickel carbonyl is between 50·6 and 55·6 cals. Hence the detonation observed by Mond might result from the reaction:

$$Ni(CO)_4 = Ni + 2CO_2 + 2C.$$

The fact that nickel carbonyl was thus reported to be explosive, together with the explanation of its explosibility offered by Berthelot, strengthened the belief in its great instability and deterred experimenters from working with it, in any case at temperatures above its boiling point.

The authors observed that when nickel carbonyl was suddenly heated in an atmosphere of some inert gas, such as hydrogen or nitrogen, the vapour decomposed quietly with deposition of metallic nickel; there was no explosion or flash of light, and the quantity of carbon dioxide produced was so small, in most cases, as to be almost negligible. This was found to be the case even when the temperature used was as high as 130° C. Mittasch* states that he could detect no carbon dioxide when the compound decomposed below 100° C.

The explosion observed by Mond, Langer, and Quincke must therefore have been due to the presence of oxygen, and does not occur in its absence. The amount of carbon dioxide produced by the quiet decomposition is very small, so that the Berthelot reaction, though possible, only takes place to a very slight extent.

It was therefore clear that vapour-density determinations of the compound could be made in atmospheres of inert gases at temperatures much higher than 50° C. As vapour-density determinations of such a unique compound as nickel carbonyl under varying conditions would have a special interest and might well be expected to repay careful study, a large number of vapour-density determinations were made by Victor Meyer's method at temperatures between 63° C. and the boiling point of naphthalene (216° C.), in order to ascertain the effect of increasing temperature on the dissociation of the compound. The vapour-density apparatus was filled in different experiments with various dry inert gases, viz., hydrogen, nitrogen and ethylene, all carefully purified and especially freed from oxygen.

In order to trace the effect of the rapidity of gaseous admixture on the dissociation various forms of the vapour-density reservoir were employed. An atmosphere of carbon monoxide was employed to

* *Loc. cit.*

investigate the effect of the gaseous product of dissociation on the stability of the carbonyl.

It was found that the rate of admixture of the vapour with the gases had a marked effect on the dissociation, as shown by the difference in the vapour densities, when taken under similar conditions, in the various inert gases; and, further, that the presence of carbon monoxide produced the expected diminution in the amount of the compound decomposed. In order to further confirm this, two reservoir tubes of different bore were used, one having a cross-sectional area about three times that of the other (the latter will be referred to as the narrow tube). In the latter, admixture could take place much less readily than in the former; consequently the surrounding gas would be expected to have a smaller effect on the extent of the dissociation. This was also confirmed by the experimental results; the vapour density in the narrow tube is almost independent of the gas employed.

The effect of the nature of the surface on the extent of dissociation was tested by using the tubes coated internally with a film of metallic nickel deposited from the vapour of nickel carbonyl by heating. The film of nickel seemed to bring about a state of equilibrium more rapidly, so that the vapour densities determined in these tubes were lower than those in the same tubes not covered by nickel (a similar effect was observed by Mittasch).

It was found that the rate at which the liquid evaporated, as would be expected in the case of a substance which readily dissociates, had some effect on the extent of the dissociation. Hence, it was necessary, in order to get comparable results, to arrange that approximately the same time was taken for vaporisation in all the experiments made at the same temperature. A definite end-point could be observed in each case at which the gas displaced by the vapour ceased to come off, and a much slower evolution of gas took place. The experiment was stopped when the more rapid evolution of gas gave place to the slower.

The results obtained are appended in the following table (Table I).

In the fourth column the percentages of nickel-carbonyl molecules dissociated are given, calculated from the formula $P = \frac{85 - D}{3D} \times 100$, where 85 is the theoretical vapour density of nickel carbonyl and D is the observed value.

Unless otherwise stated, it is to be understood that the determination was made in a Victor Meyer's apparatus of the usual type, occasionally referred to as the wide tube.

From the figures in the above table, it is seen that the value of the vapour density, deduced from the experiments in the wide tube, is greater in ethylene than in nitrogen and hydrogen. It was also

Table I.—Vapour Densities determined by Meyer's Method.

Temperature of the bulb. °C.	Gas filling the bulb.	Vapour density found (H = 1).	Percentage of $Ni(CO)_4$ dissociated.	Remarks.
63	Nitrogen .	83·3	0·7	Very slight deposit of nickel.
		78·2	2·7	Slight deposit of nickel.
64	Hydrogen	79·2		
66	Carbon monoxide	83·9	0·15	Nickel covered tube.
		85·2		
81 (Benzene)	Nitrogen .	71·0	6·2	Slight deposit of nickel.
		72·1		
100	Hydrogen	67·3	8·8	Nickel deposited over several inches in the tube.
		67·0		
	,,	56·9	16·7	Wide tube covered with nickel.
		56·7		
	,,	79·0	2·4	Narrow tube. Nickel deposit extended about 32 cm.
		79·8		
,,	Nitrogen .	70·8	6·7	
		70·9	..	Wide tube.
	,,	75·6	4·1	Narrow tube.
,,	Ethylene .	70·6	6·8	Wide tube. Nickel deposited over 2 cm. from bottom of tube.
		70·7		
		73·7	4·3	Wide tube. Moist nickel carbonyl. Nickel deposited irregularly over about 3 cm.
		76·7		
		77·8	2·7	Narrow tube. Nickel deposit extended 22 cm.
		79·5		
,,	Carbon monoxide	85·0	0·39	Wide tube. No visible deposit of nickel.
		83·1		
		82·6	0·6	Wide tube, covered with nickel carbonyl, dried over phosphorus pentoxide.
		84·5		
		76·4	3·87	Narrow tube. Slight deposit of nickel.
		76·0		
110 (Toluene)	Nitrogen .	48·4	25·4	Distinct deposit of nickel.
		48·1		
,,	Carbon monoxide	75·2	4·4	Deposit of nickel scarcely visible.
		74·8		
129 (Amyl alcohol)	Nitrogen .	25·1	76·5	Extensive deposition of nickel over tube.
		26·4		
	Carbon monoxide	31·7	54·5	,, ,,
		32·7		
135 (Acetic anhydride)	Carbon monoxide	26·9	72·0	,, ,,
155 (Turpentine)	Nitrogen .	22·5	94·3	,, ,,
		22·5		
	Carbon monoxide	23·2	..	,, ,,
		23·16	88·8	
182 (Aniline)	Nitrogen .	23·3	89·0	,, ,,
		23·0		
	Carbon monoxide	24·4	82·8	,, ,,
		23·3	88·2	
216 (Naphthalene)	Nitrogen .	22·4	93·0	,, ,,
		22·44		
	Carbon monoxide	(20·9)		
		21·3	99·7	

noticed that the deposit of nickel in the latter two gases extended higher up the tube than in the former, and was higher in hydrogen than in nitrogen. In carbon monoxide, on the other hand, the vapour density is higher than in the gases hydrogen, nitrogen, or ethylene, the values at 100° C. being nearly normal, and the dissociation was incomplete even in aniline vapour. This demonstrates very clearly the effect of the presence of one of the dissociation products on the amount of the dissociation.

In the narrow tube, however, the values obtained at 100° C. do not seem to depend, to any great extent, on the nature of the surrounding gas, the values in carbon monoxide and in the inert gases being almost identical, which shows the great effect of rate of admixture and diffusion on the dissociation.

The amount of dissociation increases rapidly with the temperature; in nitrogen at 155° C. it is practically complete. The rate of increase in carbon monoxide is distinctly slower, the difference between the vapour densities in nitrogen and carbon monoxide at 129° C. being quite marked. Above 155° C. the results obtained are somewhat irregular; but dissociation seems to be nearly complete at atmospheric pressure, since only a small deposit of nickel could be obtained when the tube was placed horizontally and a clear part heated with the blowpipe.

A few vapour-density determinations were also made by Hoffmann's method at temperatures between 17° C. and the boiling point of aniline (182° C.) in order to observe the dissociation of the undiluted vapour. Complete dissociation is practically reached at 182° C., but even then the application of a pointed flame to a clear portion of the tube produced a slight deposit of nickel, so that traces of nickel carbonyl were still present.

The results are given in the appended table (Table II).

The results of the experiments in the narrow tube are given in the fifth column for the sake of comparison, the phenomena in this case being practically the dissociation of the vapour in contact with its own dissociation products. The dissociation is clearly greater under reduced pressure, as might be anticipated.

The general results of the vapour-density determinations are readily seen from the curves in fig. 1 (p. 433).

Having thus found that the vapour of nickel carbonyl was much more stable at elevated temperatures than had hitherto been suspected, we resolved to examine the stability of the liquid under pressure, and if possible make observations on it as far as its critical point.

Small sealed tubes, from one-half to one-third full of nickel carbonyl, were heated to 200° C. without bursting. A small quantity of nickel was deposited on the first heating, and it was found that its quantity was not appreciably increased on repeating the operation. On

standing a sufficient length of time at the ordinary temperature the nickel gradually dissolved again, although it was covered by the excess of liquid. This proved in an interesting manner the ease with which the reaction between the deposited nickel and carbon monoxide is reversible.

The amount of the nickel deposition was notably greater when the liquid was heated in contact with mercury, so that the use of the Cailletet pump for the examination of the critical phenomena was not practicable.

Rough measurements of the critical temperature were made by observing the temperature at which the meniscus disappeared and

Table II.—Vapour Densities of Nickel Carbonyl by Hofmann's Method.

Temperature of bath.	Vapour density.	Pressure in mm. of mercury.	Percentage of nickel carbonyl dissociated.	Percentage dissociated at atmospheric pressure.	
				In nitrogen.	In carbon monoxide.
°C.					
17	85·6	67·7	0·0		
35	81·8	161·4	0·13		
(Ether)					
78	62·6	217·8	11·9	4·1 at 100° C. in narrow tube	3·87 at 100° in narrow tube
(Alcohol)					
129	25·2	389·4	79·1		
(Amyl alcohol)	23·8	365·6	85·7	76·5 wide tube	54·5 wide tube
182	21·5	349·0	98·4		
(Aniline)	20·8	320·6	..	Practically complete in wide tube.	
	21·7	356·0	95·7		

reappeared, when a quantity of the liquid was alternately heated and cooled in an exhausted and sealed off piece of glass tube of 2—3 mm. bore. The tube was heated side by side with a thermometer in a glycerine bath, and was so arranged that it could be inverted at will. The temperatures at which the meniscus disappeared and reappeared in the course of many repeated observations with different samples of material, ranged between 191° C. and 195° C.

It appears from the observations made with such tubes that the most reliable results were obtained the first time they were heated, the meniscus being better defined and disappearing more sharply than on subsequent occasions. The blurred effect noticed on a repetition of the experiment is due to a somewhat greater amount of carbon monoxide being present.

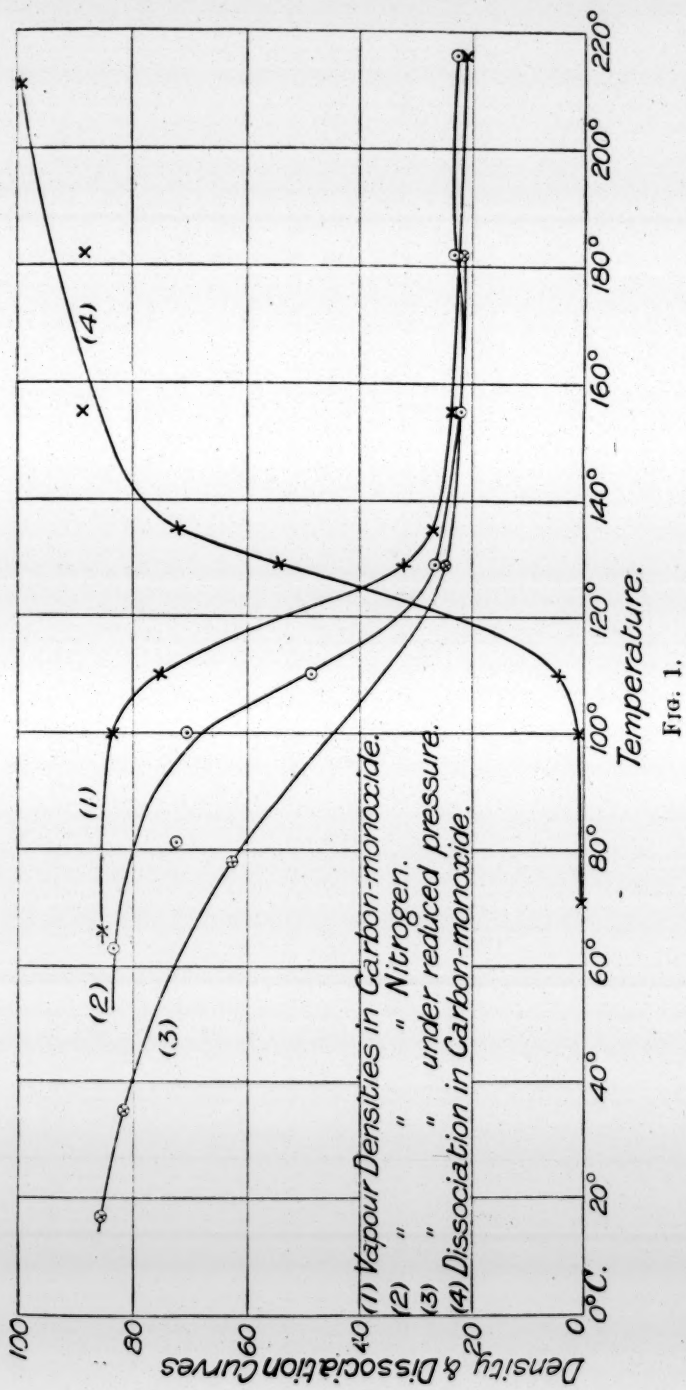


FIG. 1.

The presence of carbon monoxide would be expected to cause a lowering of the temperature at which the meniscus disappeared, and this expectation was realised in the course of the experiments. Hence the temperature observed in the first experiment is probably below the

true critical temperature. The presence of 4—5 per cent. of carbon monoxide with its low critical temperature 128° abs. would be expected to lower the temperature at which the meniscus disappears by about 5° C. That this quantity of carbon monoxide might be present at any time is shown by the following experiment:—

Volume of tube, 1·35 c.c.
„ nickel carbonyl left after heating to 193° C. = 0·437 c.c.
Amount of nickel deposited = 0·0147 gramme.
„ carbon monoxide liberated = 0·0284 gramme = 5·02 per cent. of the residual nickel carbonyl.

Hence it is very probable that the true critical temperature is about 200° C.

Comparative experiments made with pure ethyl ether in similar tubes, gave a critical temperature of 193° C. The accepted value for the critical temperature for ether being 194°·5 C., the observed temperature for nickel carbonyl cannot be far removed from the correct value.

The formula $T_b = 0{\cdot}66\ T_c$, where T_b is the absolute boiling point and T_c the absolute critical temperature, should be applicable to the case of nickel carbonyl since it is applicable to ether, a liquid which has a critical temperature of approximately the same value. Taking the boiling point to be 43°·3 C., the critical temperature calculated from the above relation would be 201°·4 C., which agrees very well with the value which was found as a probable minimum critical temperature.

Although it had not been surmised that nickel carbonyl could ever stand heating above 100° C., nevertheless Mond and Nasini* calculated the hypothetical critical temperature of nickel carbonyl from the results of their experiments on its coefficient of expansion by means of a formula given by Thorpe and Rücker and found the value 151° C. In a similar manner Ramsay and Shields† calculated the critical temperature from the temperature coefficient of the molecular surface energy. The value deduced in this way is 182°·8 C. It therefore appears that the hypothetical critical temperature calculated by either of these methods falls considerably below its actual value.

Rough indications of the critical pressure were obtained by introducing into the sealed tube containing the nickel carbonyl a small tube of very fine bore, closed at one end and having a small globule of mercury introduced at the other, to act as a manometer. The position of the globule was observed at the ordinary temperature, and again at the critical point. The volume of the air in the small tube occupied about one-thirtieth of its original volume at the latter

* 'Zeit. Phys. Chem.,' 1891, vol. 8, p. 150.
† 'Jour. Chem. Soc.,' 1893, vol. 63, p. 1108.

temperature, so that the pressure in the tube seems to be rather more than 40 atmospheres at the critical point. On cooling, the indicating globule remained permanently displaced some distance up the tube, showing the existence of a pressure developed by the decomposition of the nickel carbonyl. On standing for some time the whole of the nickel disappears, and the carbonic oxide pressure disappears.

The pressure on cooling seemed to be about ten atmospheres, hence the critical pressure would be about thirty atmospheres. Later it will be shown that this is near the actual value.

The critical constants of the compound being known, together with the boiling point, it is possible to calculate a vapour-pressure curve. It was, however, thought better to determine the vapour pressure at a number of temperatures below the boiling point of the liquid, by the static method, and from this curve by extrapolations to deduce the values for higher temperatures.

A wide barometer tube (about 0·7 cm. diameter) was carefully cleaned, dried, and drawn off to a fine capillary tube at one end. The tube was then placed upright in a vessel of pure dry mercury and exhausted thoroughly with a Fleuss pump. A small tube full of nickel carbonyl was now introduced at the bottom of the tube and the whole then exhausted again, while surrounded by a freezing mixture, in order to get rid of all adhering air, and finally sealed off rapidly at the fine capillary. By this method of procedure only a very small amount of decomposition took place during the sealing off, as indicated by the very slight deposit of nickel. The pressure was then read off by means of a kathetometer, while the tube was surrounded by a bath kept at various constant temperatures.

The results are appended below, together with those obtained by Mittasch* by the dynamic method.

Dewar and Jones.		Mittasch.	
− 9° C.	94·3 mm.	2°·05 C.	133·1 mm.
− 7	104·3	7 ·5	170·5
− 2	129·1	15 ·27	238·2
0	144·5	20 ·2	294·3
+10	215·0	24 ·26	349·7
+16	283·5	29 ·52	444·2
+20	329·5	34 ·29	532·6
+30	461·9	39 ·97	647·2

The values for − 9° C. and + 30° C. give the following Rankine formula for the relation between the pressure p in millimetres of mercury and the absolute temperature T. $\text{Log}\, p = 7{\cdot}355 - 1415/T$. At 200° C. (about the critical temperature) the pressure calculated from this equation is 30·4 atmospheres. Taking the results obtained

* *Loc. cit.*

by Mond and Nasini, viz., the boiling point 43° C. 751 mm. and the pressure at 20° C. 338·7 mm., a similar expression, $\log p = 7{\cdot}281 - 1392/T$, is obtained.

Taking Mittasch's pressures for 2° C. and 40° C. the Rankine obtained is :—

$$\log p = 7{\cdot}781 - 1555{\cdot}7/T.$$

The boiling points calculated from these formulæ are given below :—

Mond.....	43°·3 C.
Mittasch	44°·4
Dewar and Jones	43°·2

In order to further confirm our vapour-pressure determinations, we made a determination of the boiling point of some carefully dried and redistilled nickel carbonyl. With the barometer at 769 mm. the liquid boiled at 43°·2 – 43°·33 C.

It would therefore appear that our vapour tension curve is the more accurate. The following curves, fig. 2, illustrate very clearly the extent of the deviations at different temperatures.

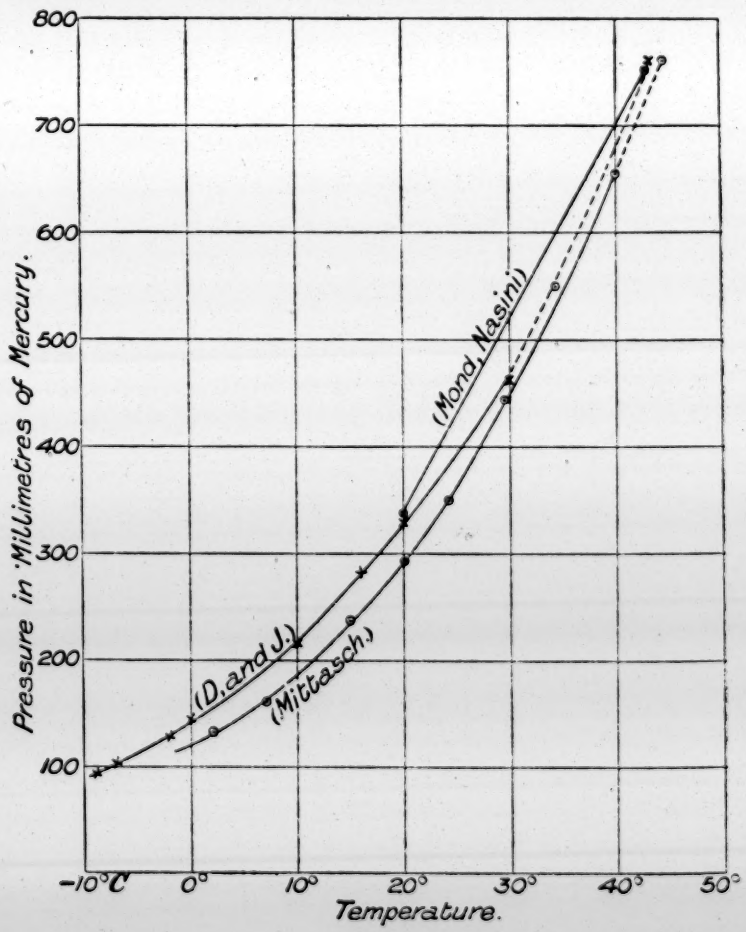


Fig. 2.

The latent heat of vaporisation of $Ni(CO)_4$, is 38·1 calories per gramme, and the Trouton constant (molecular latent heat divided by the absolute boiling point) is 20·6, its value for ether being 22. The number obtained by dividing the absolute critical temperature by the critical pressure, which is proportional to the volume of the molecule, Van der Waal's constant b, is 15·5; the similar number for carbon monoxide is 3·7, so that nickel carbonyl according to theory has a molecule 4·2 times larger than carbon monoxide. The molecular volume of nickel carbonyl at its boiling point is 136, as compared with 110 for ether. The critical density appears to be about 0·46, while that of ether is only 0·25. If the liquid densities of Mond and Nasini* are taken with the critical data, then the Waterston formula $v = 2·0398 - 0·5667 \log (198·7 - t)$ fits in very well with the results. A similar formula for ether given by Avenarius is:—

$$v = 3·19 - 0·802 \log (191 - t).$$

The molecular volume of nickel carbonyl at its boiling point is 136, subtracting from this 7·2 for the nickel atom, we have 32·2 as the volume of each molecule of carbon monoxide in the molecule. Now liquid carbon monoxide at its boiling point has the molecular volume of 35, so that contraction would take place if liquid carbon monoxide could combine with nickel. The heat of formation of nickel carbonyl is about four times greater than that of the liquefaction of the equivalent amount of carbonic oxide under normal conditions.

The experiments described above show clearly that nickel carbonyl is a substance admirably suited for the demonstration of the phenomena of dissociation. Great care must, however, be taken in handling the substance, owing to its poisonous properties when inhaled. It also forms an excellent illustration of a reversible reaction, and the following experiments serve to illustrate the way in which it may be used for this purpose.

A number of carefully dried tubes were exhausted by means of a Fleuss pump, and were then filled with a mixture of 10 per cent. nickel carbonyl vapour and 90 per cent. carbon monoxide, at pressures of 50, 100, 226, 304, 396, 504, and 624 mm. of mercury respectively. These tubes were then heated in a bath until nickel began to deposit; the tube under observation was then kept at that or a slightly higher temperature for about half-an-hour, and afterwards tested for the presence of nickel carbonyl by heating a clean portion of the tube with a fine pointed flame, and in this way the presence of even a very small trace of nickel carbonyl was immediately detected by the formation of a bright mirror of nickel on the hot part of the tube. The tube at 50 mm. pressure did not deposit nickel when placed in alcohol vapour,

* *Loc. cit.*

but did so at 100° C., and, after heating for a few hours at this temperature, no nickel carbonyl could be detected in the tube. After standing for a few days, however, nickel was deposited when a clean portion of the tube was heated with a pointed flame, thus showing that nickel carbonyl had been regenerated. In the 100 mm. tube nickel was still deposited after heating for two days at 100° C.; all the nickel carbonyl was found to have disappeared after the tube had been heated for some time to 130° C. Nickel was deposited in the tube at 226 mm. pressure at 130° C., in the 301 mm. tube at 158° C., and in the other tubes at slightly higher temperatures. In the two tubes at the highest pressures there was a considerable quantity of the carbonyl present after heating for an hour at 160° C. All the tubes in which the nickel carbonyl had been so far destroyed that no visible deposit of nickel could be obtained on heating a clean portion of the tube with a small flame, after standing for a few days contained enough of the carbonyl to be readily detected by the above test.

Another form of experiment suitable for demonstration proved the reaction proceeded rapidly at the ordinary temperature, and with a measurable velocity at low temperatures, even when the pressure of the carbonic oxide atmosphere was below 200 mm. A large bulb of about 200 c.c. capacity was connected to a mercury manometer of small bore (so that the movements of the mercury in the manometer were proportional to the changes of gas concentration in the bulb). The bulb was highly exhausted and then filled with pure nickel carbonyl vapour to a pressure of 51 mm. of mercury at 15° C. After heating for about an hour to 100° C. the pressure, measured after cooling, had risen to 143 mm., corresponding to a decomposition of about 60 per cent. of the nickel carbonyl present. Heated in a glycerine bath to 154° C. the pressure reached 198 mm., corresponding to practically complete decomposition, which ought to develope a total pressure of 204 mm.

On rapidly cooling the bulb and allowing it to stand at the ordinary temperature the pressure fell, at first, about 3·2 mm. in an hour, then after two days it had fallen to 120 mm., or about 55 per cent. had recombined, after four more days the pressure was 97 mm., or about 60 per cent. had recombined, after standing four weeks some of the deposited nickel remained unattacked.

The bulb was again heated to 150° C., so as to deposit all the nickel on the lower part of the tube, and the pressure now rose again to 200 mm. The lower part of the bulb where the nickel had deposited, was now immersed in liquid air, when it was observed that still a small but distinct diminution in pressure took place after some hours. Liquid carbonic oxide did not, however, appear to react with nickel reduced from the oxide by hydrogen.

The volatile iron carbonyl has been made the subject of a number of similar observations, dealing with its physical properties and chemical stability, which will be discussed in another communication.

"An Enquiry into the Variation of Angles observed in Crystals, especially of Potassium-Alum and Ammonium-Alum." By Professor H. A MIERS, M.A., D.Sc., F.R.S. Received March 10,—Read March 26, 1903.

(Abstract.)

Corresponding angles measured on different crystals of the same substance usually differ slightly. On cubic crystals the theoretical angles are known. Pfaff professed to have established that only those cubic crystals which display birefringence exhibit divergence from the theoretical angles, but Brauns showed that in lead nitrate, ammonia-alum, and spinel, for both isotropic and birefringent crystals alike, the octahedron angle may differ by as much as 20′ from that of the regular octahedron.

The author has endeavoured to trace the changes of angle upon one and the same crystal during its growth by measuring it at intervals without moving it from the solution in which it is growing. This is accomplished by means of a new telescope-goniometer in which the crystal is observed through one side of a rectangular glass trough, and the changes in the inclination of each face are followed by watching the displacements of the image of a collimator slit viewed by reflection in it. The crystal is held by a platinum clip which it envelopes as it grows. Small movements of the image are followed by means of a special micrometer-eyepiece which accurately measures the magnitude and direction of the displacement.

Examined in this way an octahedron of alum (ammonium or potassium) is found to yield not one but three images from each face; and closer inspection shows that the crystal is not really an octahedron, but has the form of a very flat triakis octahedron. It often happens that of the three faces which nearly coincide, one is large and the remaining two very small, so that of the three images one is bright and the others are very faint, and can only be discerned with difficulty; in such a case the crystal as measured in the ordinary way would appear to be an octahedron whose angle differs from the theoretical value by a few minutes.

When a growing crystal of alum is watched for several hours or days, it is found that the three images yielded by an apparent octahedron face continually change their position; one set fades away and is replaced by another set, which are generally more widely

separated than those which they succeed. The images move in three directions inclined at 120° to each other, and indicate that these faces always belong to triakis octahedra. The point in which the lines of movement intersect within the field of view of the telescope would, therefore, be the position of the image reflected from the true octahedron face. Measured in this way the octahedron angle of alum is found to be the theoretical angle 70° 31¾′.

The images do not move continuously, but *per saltum*, indicating that the reflecting planes are vicinal faces which probably possess rational indices, and must, therefore, be inclined at certain definite angles to the octahedron face; but the indices are very high numbers.

Observations upon sodium chlorate, zinc sulphate, magnesium sulphate, and other substances, show that other crystals exhibit the same behaviour. The faces of a crystal are in general not faces with simple indices, but vicinal planes slightly inclined to them, and they change their inclinations during the growth of the crystal; they also change their inclinations when the crystal is immersed to a greater or less depth in the solution.

Every point within a crystal has at some time been a point on the surface, and has been subject to the conditions of equilibrium between crystal and solution which prevail there. It is believed by the author that a study of the vicinal planes and of the liquid in contact with them, may lead to some understanding of these conditions.

In order to ascertain the composition of the liquid, attempts were made to determine its refractive index by means of total reflection within the crystal. This appears, indeed, to be the only method which can give direct information concerning the ultimate layer in contact with the growing face, and it is somewhat remarkable that it has not been applied before. Considerable difficulty was experienced in making this measurement, but ultimately good readings were obtained, which gave the value 1·34428 as the refractive index in sodium light, at 19° C., of the liquid in contact with a growing crystal of alum. The refractive indices of a series of solutions of known strength, ranging from dilute to supersaturated, having been previously measured, the above index was found to correspond to a liquid containing about 10·80 grammes of alum in 100 grammes of solution. A saturated solution at 19° C. was found to have the refractive index 1·34250, and to contain about 9·01 grammes of alum in 100 grammes of solution.

Sodium chlorate was examined in the same way; it was found that the liquid in contact with a growing crystal has at 19° C. the index 1·38734, and contains about 47·8 grammes of salt in 100 grammes of solution; a saturated solution of sodium chlorate at 19° C. has the index 1·38649, and contains about 47·2 grammes of salt in 100 grammes of solution.

The liquid in contact with a growing crystal of sodium nitrate has at 19° C. the index 1·38991, and contains about 48·45 grammes of salt in 100 grammes of solution; a saturated solution at 19° C. has the index 1·38905, and contains about 48·1 grammes of salt in 100 grammes of solution.

In each case the liquid in contact with the growing crystal is slightly supersaturated. It was not found to exhibit double refraction even in the case of sodium nitrate. No experiments seem to have previously been made upon the nature of this liquid.

G. Wulff has suggested that vicinal faces are due to concentration streams in the solution. In order to test this view, crystals of alum were measured after growing for several hours in solution kept continually agitated in order to eliminate the action of the concentration streams. Almost no effect was produced upon the angles of the vicinal faces.

In sodium chlorate and sodium nitrate the solute is about 45 times more dense in the crystal than in the adjacent liquid. Now planes with high indices in a space-lattice contain fewer points in unit area than planes with simple indices. The author suggests that vicinal faces grow upon a crystal in preference to simple forms because the crystallising material descends upon the growing face in a shower which is not very dense.

"On the Dependence of the Refractive Index of Gases on Temperature." By GEORGE W. WALKER, M.A., Fellow of Trinity College, Cambridge. Communicated by Professor J. J. THOMSON, F.R.S. Received February 26,—Read March 26, 1903.

(Abstract.)

The investigations of Professor Mascart on this subject are perhaps the most extensive of any up to the present time. He examined the effect in several gases, and found that in general the temperature coefficient exceeded the theoretical coefficient given by Gladstone and Dale's law. The range of temperature was, however, comparatively small, and his results for air do not agree with those of Lorenz, von Lange, and Benoît. In fact these four observers disagree. Lorenz and Benoît found a coefficient agreeing with the above law, while von Lange obtained a coefficient less than the theoretical value.

A repetition of the measurements therefore seemed desirable. The gases examined were air, hydrogen, carbon dioxide, ammonia, and sulphur dioxide. The range of temperature was from 10° C. to

100° C. The method used was the well-known one of Jamin, but special precautions were taken to obtain accuracy, and to be sure that the gas had not changed in composition during the various changes of pressure and temperature to which the containing tubes were subjected. An accuracy of about one part in 600 has been obtained.

The results are briefly shown in the following table, and it will be observed that the temperature coefficients obtained are substantially less than those obtained by Mascart.

Absolute Value of μ for the D line at 760 mm, and 0° C.

Observer.	Air.	Hydrogen.	Carbon dioxide.	Ammonia.	Sulphur dioxide.
Mascart.....	1·0002927	1·000139	1·000454	1·000379	1·0007038
Lorenz.....	—	1·000139	—	1·000373	—
Ketteler	—	1·000143	1·000449	—	1·000686
Dulong	1·000294	1·000138	1·000449	1·000385	1·000665
Walker.....	1·0002928 ±3	1·0001407 ±15	1·0004510 ±5	1·0003793 ±5	1·0006758 ±4

Temperature Coefficients of Refractive Index.

	Air.	Hydrogen.	Carbon dioxide.	Ammonia.	Sulphur dioxide.
Coeffi. of vol. expansion.	0·00367	0·00366	0·00371	0·00382?	0·00390
Mascart	0·00382	0·00378	0·00406	—	0·00460
Walker.....	0·00360 ±3	0·00350 ±3	0·00380 ±3	0·00390 ±3	0·00416 ±2

"On the Evolution of the Proboscidea." By C. W. ANDREWS, D.Sc. Communicated by Professor E. RAY LANKESTER, F.R.S. Received March 5,—Read March 26, 1903.

(Abstract.)

Until the author's recent discoveries of primitive Proboscidea in the Middle and Upper Eocene formations of the Fayum, Egypt, the oldest known members of this mammalian order were *Dinotherium curieri* and *Tetrabelodon angustidens*, from the base of the Miocene in France. The new Egyptian fossils not only reveal for the first time the early history of the order, but also provide more satisfactory material for the discussion of its evolution than has hitherto been available.

The most important changes in the Proboscidea occur in the skull, mandible, and dentition.

Owing to the increase in the size of the tusks and to the presence of the proboscis, the facial region of the skull becomes shortened, and at the same time the premaxillæ become wider. The presence of the proboscis also accounts for the position of the external nares. The demand for a greater surface of attachment for the muscles supporting a skull rendered heavy by the tusks and trunk, is met by the great development of the diploë in certain of the cranial bones, resulting in the enormous expansion of the forwardly sloping occipital surface. The maxillæ become greatly enlarged concomitantly with the increase in the size and degree of hypselodonty of the molars. At the same time the zygomatic arch becomes weaker and the jugal takes a smaller share in its composition.

The mandible is at first short and stout, with a massive symphysis. Afterwards it becomes more and more elongated as the stature of the animals increases; and this elongation is for the most part effected by the lengthening of the symphysial region, though the backward rotation of the ascending ramus tends to the same end. The prolongation of the mandible beyond the premaxillæ must have been covered by a proboscis-like structure composed of the upper lip and nose, probably more or less prehensile at its extremity. The lengthening of the mandible seems to have reached its maximum degree in the Middle Miocene, after which it again became shortened by the reduction of the symphysis, while the fleshy and now mobile proboscis was left behind as the sole organ of prehension.

In the upper dentition the chief changes are the loss of incisors Nos. 1 and 3, and the great increase in size of incisor No. 2, which eventually forms the great tusk characteristic of the later Proboscidea. The canines are soon lost. In the earliest forms, some at least of the cheek-teeth (milk-molars) are replaced by premolars in the usual

manner, and these teeth remain in wear simultaneously with the true molars; but in later forms no vertical succession takes place, and as the milk-molars are worn they are shed, being replaced from behind by the forward movement of the molars. Of these also the anterior may be shed, until at length in old individuals of the later types the last molar is alone functional. The gradual increase in the complexity of the proboscidean molars is one of their most striking characteristics. All stages can be traced between the simple, brachyodont, bilophodont (quadritubercular) molars of *Moeritherium* (Middle Eocene) to the extraordinarily complex type of tooth found in *Elephas*. Thus in *Palæomastodon* (Upper Eocene) the molars are trilophodont, and the same is true of the first and second molars of *Tetrabelodon* (Miocene), in which, however, the last molar is complicated by the addition of further transverse crests. In the Stegodonts of the Siwalik Hills (Pliocene) a further increase in the number and height of the crests takes place, and the whole crown of the tooth is more or less covered with a thick coat of cement. Still later, the transverse crests become highly compressed laminæ united by cement, and these are as many as twenty-seven in number in the Pleistocene *Elephas primigenius* and the recent *E. indicus*.

The evolution of the lower molars corresponds with that of the upper molars. Of the lower incisors the middle and outer pairs (Nos. 1 and 3) are soon lost, but the second pair remains functional for a long geological period. When the symphysis becomes shortened, these incisors are sometimes retained as vestiges (*e.g.*, in *Mastodon americanus*), but in the genus *Elephas* they have completely disappeared.

"A Comparative Study of the Grey and White Matter of the Motor Cell Groups, and of the Spinal Accessory Nerve, in the Spinal Cord of the Porpoise (*Phocæna communis*)." By DAVID HEPBURN, M.D., and DAVID WATERSTON, M.A., M.D. Communicated by Sir WM. TURNER, F.R.S. Received January 23, —Read March 12, 1903.

(Abstract.)

Recent advances in our knowledge of the arrangement of the motor cells in the anterior cornua of the spinal cord of man have been made almost entirely by the study of the changes produced in these cells by the division or removal of limbs or parts of limbs in the human subject, and very little has, as yet, been done to elucidate this subject by the comparative method of investigation.

The authors considered it probable that much information might be obtained by the careful study of the cell groups in the spinal cord of a mammal differing markedly in its musculature from man, and as no previous observations on similar lines had been made in the same fulness on the spinal cord of any of the Cetacea, they describe the results of an examination of the cell groups in each segment of the cord of a member of this class, *Phocæna communis*. The investigation was carried out by obtaining a very recently captured specimen and at once preserving its tissues by injecting into its blood-vessels a solution of formalin, a method which has the advantage of preserving the natural configuration of the enlargements of the cord. A number of sections were prepared for the microscope by different methods from each segment of the whole cord, and typical sections were selected and photographed.

The principal features in which the musculature of the porpoise differs from that of man are the almost entire absence of a hind limb, the reduced musculature of the upper limb, and the possession of a large and flexible tail acted upon by powerful muscles, with some other differences noted in the text.

The segments of the cord giving origin to the nerves supplying these parts were compared with corresponding segments of the human cord. The groups of motor cells were found to be clearly differentiated from one another, and striking changes were found to occur in the shape of the grey matter and in the cell-groups as we passed from one segment of the cord to another, pointing clearly to a connection between the character of the part supplied with motor nerves from any segment, *e.g.*, limb, trunk muscles, genital muscles,—and the arrangement of the anterior horn cells in that segment.

The area of the grey matter and of the different columns of white matter was also determined at each segment.

The authors also describe some hitherto unrecorded features in the minute structure of the cord of this animal, especially the position of the nucleus of the spinal accessory nerve, and a detached mass of grey matter, probably corresponding to the vesicular column of Clark, in the lumbo-sacral region of the cord.

"Solar Prominence and Spot Circulation, 1872—1901." By Sir NORMAN LOCKYER, K.C.B., F.R.S., and WILLIAM J. S. LOCKYER, Chief Assistant, Solar Physics Observatory, M.A. (Camb.), Ph.D. (Gött.), F.R.A.S. Received March 17,—Read March 26, 1903.

[PLATES 6 AND 7.]

In our former communications* referring to the connection between solar, meteorological, and magnetic changes, some of the results obtained by the reduction of the solar prominences, as observed by Professor Tacchini at Rome, were described. It was stated that the curve representing the variation of percentage frequency of the prominences for the whole limb of the sun indicated that in addition to the main epochs of maxima and minima coinciding in time generally with those of the maxima and minima of the total spotted area, there were also prominent subsidiary maxima and minima.

Further, dividing the sun's limb into zones of 20° in width from the equator, with a polar zone of 10°, and dicussing each zone separately, the variation of the prominence percentage frequency about the equator was found to be very different from that in the higher latitudes, the former changing with the spots, and the latter exhibiting sudden outbursts just previous to the epochs of sunspot maxima, followed and preceded by comparatively long intervals of quietude.

In the present communication, the prominence observations have been discussed from a different point of view, in order to trace out, if possible, the heliographic latitudes of the chief centres of action of prominence disturbance. In this way it could be determined whether such movements are subject to some periodic law, in which case it would be possible to increase our knowledge of the circulation of the solar atmosphere in regions outside those in which sunspots alone have, up to the present, been employed.

The changes of latitude of the zones which contain the centres of sunspot disturbances were first pointed out by Carrington,† whose fine series of observations led him to discover "a greater contraction of the limiting parallels between which spots were formed for the two years previous to the minimum of 1856, and soon after this epoch the apparent commencement of two fresh belts of spots in higher latitudes north and south, which have in subsequent years shown a tendency to coalesce, and ultimately to contract as before to extinction."

The study of the subject was taken up later by Spoerer,‡ who

* 'Roy. Soc. Proc.,' vol. 70, p. 502; vol. 71, pp. 134 and 244.

† 'Observations of the Spots on the Sun from November 9, 1853, to March 24, 1861, made at Redhill,' p. 17.

‡ 'Beobachtungen der Sonnenflecken von Oct., 1871—Dec., 1873, und von Jan.,

corroborated Carrington's results and extended the discussion of the observations up to the end of the year 1879.

The result of these two investigations showed that at sunspot maximum there was only one zone in each hemisphere in which spots were situated, the centre of this being about 18° N and S, while at minimum there were two zones existing simultaneously in each hemisphere; the older cycle dying out in the zone, the centre of which was situated in low latitudes, and the new one commencing in high latitudes, its centre being about latitude ± 30° to ± 35°.

Later observations extending up the present year have further corroborated these general deductions, for each hemisphere, and we are now quite familiar with this cycle of sunspot latitude variation.

In the present investigation, the fact has been brought out that the prominences also undergo an apparently regular variation of latitude throughout a period of about eleven years concurrently with the spots.

For the purpose of our inquiry, the object of which has been stated above, we have discussed independently of each other, two fine series of prominence observations, one made by Tacchini at Rome extending from 1872 to 1900, and the other by Ricco and Mascari at Catania from 1881 to 1901.

Both these series were handled in the same way, and both indicated similar changes of latitude of prominence action, showing that the variations recorded were real and not due to any personality of the observer or difference in the method of observation.

The data for the discussion of the solar prominences as observed by Tacchini have been taken from the same source as before,* while those of Ricco and Mascari are published in and have been extracted from the same volumes.

We may here take the opportunity to express our thanks to Professor Ricco, with whom we have been in communication, and who has very kindly forwarded for our use some unpublished data concerning his prominence observations and reductions.

The method of reduction adopted was to determine for each year the percentage frequency of prominence activity for every 10 degrees of solar latitude north and south. A series of curves was next drawn, one for each year, the abscissæ representing the latitudes of prominences north and south, and the ordinates their percentage frequency. It was then found that the centres of prominence activity, or, in other words, the maxima of the curves, were sometimes single, sometimes double, and in one or two cases even triple in each hemisphere. This suggested that just as sometimes there are two

1874—Dec., 1879.' 'Publicationen des Astrophysikalischen Observatoriums zu Potsdam,' Band I and II.

* 'Società Spettroscopisti Italiani,' vol. 1, 1872, to vol. 29, 1900.

zones of spots existing at one time, so there might be one, two, or occasionally three zones of prominences in existence in each hemisphere simultaneously.

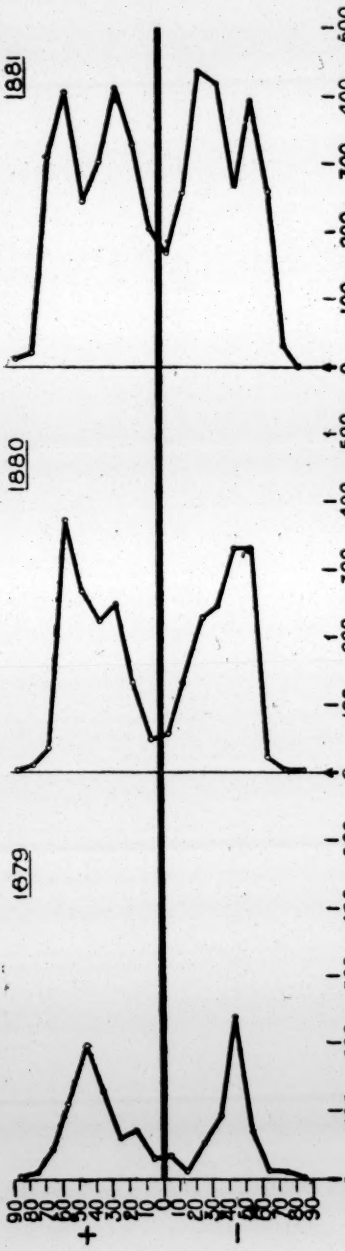


Fig. 1.—Curves showing the variation of the percentage frequency of prominence activity for every 10° of latitude for the years 1879—1881, from Tacchini's observations.

Further, a close examination of the whole set of curves with reference to these points of maxima made it possible not only to study the changes of latitude of these points from year to year and their positions when commencing to develop or about to disappear, but the intensity of these centres in relation to each other.

The accompanying illustration (fig. 1) shows the curves drawn for the years 1879, 1880, and 1881, from the observations of Tacchini, and serves as an example of the curves that have been discussed; they exhibit the change from a single to a double centre of activity in each hemisphere.

Thus in 1879, there was a prominence maximum in each hemisphere at latitudes $\pm 50°$. In the next year (1880), both the maxima had retreated further away from the equator, namely to latitudes $\pm 60°$, while another centre of disturbance began to make itself apparent at latitudes $\pm 30°$. In the year 1881, both centres in each hemisphere were strongly marked and became of about the same intensity, their mean latitudes in each hemisphere being about $\pm 30°$ and $\pm 60°$. These curves thus indicate that during these three years, the direction of motion of these centres of activity tends polewards or away from the equator.

By examining both series of observations made by Tacchini and Ricco and Mascari, and analysing the positions of the principal and

subsidiary maxima for the whole period covered by the observations, the results illustrated graphically in Plates 6 and 7 were obtained.

In these figures the facts are brought together for each hemisphere separately. The medials of the lines (curves A and B) show the heliographic latitudes of the centres of prominence action; the thickness of these lines represents the relative percentage frequency of prominence action.

For the sake of comparison, three other curves for each hemisphere are given. The first (curve C) shows the mean heliographic latitude of spotted area for each hemisphere. For the construction of these, the values, since 1873, have been extracted from the Greenwich Reductions,* but previous to that date the values have been obtained from Mr. Marth's reductions,† and those completed at the Solar Physics Observatory from measures supplied by Professor Backlund, of the Wilna Observatory.

The next curve (curve D) illustrates the variations of the percentage frequency of prominence action for each hemisphere taken, as a whole, and is similar to those given in our former papers.

The last curve (curve E) shows the variation of the mean daily area of sunspots from year to year, also for each hemisphere.

Referring now to the changes of latitude of the prominence centres of activity, both series of curves for the north as well as for the south hemisphere exhibit the same general features.

The first conclusion illustrated by the curves is that prominence activity in the main has a poleward drift, that is, the change of position of the zones of activity is in the direction from low to high latitudes. In some years, the centres of activity appear to form two zones in each hemisphere at about latitudes $\pm$ 24° and $\pm$ 50°, which eventually amalgamate at about latitude $\pm$ 40° and move polewards, fading out in about $\pm$ 70° to $\pm$ 80°. As this zone disappears in high latitudes a new zone at about latitude $\pm$ 20° begins, and this after a few years becomes associated with another zone in about latitude $\pm$ 50°, and eventually amalgamates with it.

The epochs at which these different zones come into play in relation to the general curve of prominence activity for the whole hemisphere are as follows: From a little after the maximum of prominence activity to just before the minimum, two zones in the latitudes $\pm$ 24° and $\pm$ 50° are in existence and of decreasing intensity. Before the minimum is reached these two zones amalgamate in about latitude $\pm$ 40°. At the minimum there is only one zone, and this of small intensity. Between the minimum and the following maximum this zone rapidly takes a northern movement, increasing in intensity; a new outburst

* 'Spectroscopic and Photographic Observations made at Royal Observatory, Greenwich, 1884,' and after.

† MSS. at Royal Society.

occurs in a zone nearer the equator (latitude ± 24°), which also increases rapidly in intensity.

After these general statements, we now refer to some details showing that there are some variations from the above generalisation.

For these details the curves deduced from both sets of observations made by the different observers are so very similar that it does not matter which are examined.

Attention may first be drawn to certain differences between the curves representing the latitude variation for the two hemispheres. It will be noticed that for the period 1872—1882, the curves for both hemispheres are very similar. We next consider the period 1880—1893. Here there are differences between the two hemispheres. The curve for the northern hemisphere resembles very closely that for the preceding period, but it differs somewhat from its corresponding curve for the southern hemisphere. The corresponding northern zone in latitude 45° is missing from the southern hemisphere, while a zone of activity nearer the equator about latitude 24° is present. Further, the polar zone for the southern hemisphere continues to be prominent for two years longer than that in the opposite hemisphere.

In the succeeding curves, which extend from 1891—1901, both hemispheres are more or less similar, and both resemble in a greater degree those for the southern hemisphere for the period 1880—1893 than those for the period 1872—1882.

Although the Roman and Sicilian observations give nearly identical curves, hemisphere for hemisphere, the apparently regular cycle of change of latitude which was operative for the northern hemisphere 1872—1893, and for the southern hemisphere 1872—1882, does not seem to have been so exactly maintained in late years; more irregular still perhaps is the last cycle commencing in the year 1892. Hence, there seems reason to believe that the prominence circulation is not quite the same for each cycle, and this may in some way be due to a longer solar period such as that of about 35 years.

But it is important to state that our deductions may be partially incomplete owing to the difficulty of determining sometimes whether a new centre of action has been formed or the position of an old one changed. Further, account must be taken of the fact that the material discussed does not represent the record of the percentage frequency of prominences determined from observations made on the disc of the sun (now rendered possible by the Janssen-Hale-Deslandres method), but one obtained from observations of the phenomena occurring only at the limb of the sun. The close agreement between the observation of the different observers shows nevertheless that this latter method is of great value.

Another important series of prominence observations is that made

by Father A. Fényi, S.J., who has published* the individual observations, and the reductions of the positions and frequency of prominences observed at Haynald Observatory for the years 1884 to 1890 inclusive. He gives curves constructed somewhat after the manner adopted in the present enquiry, as illustrated above, in fig. 1. A comparison of the points of maxima from his curves with those of Tacchini and Ricco and Mascari for the period common to all three sets of observations is made in the following tables, each hemisphere being given separately. The vertical columns show, for each year, the heliographic latitudes of the points of maxima, and an asterisk (*) is placed against the one which is the more or most prominent in each hemisphere; when there are two, and they are of equal intensity, this symbol is attached to each, while in the case of only slight indications of maxima the latitude is enclosed in brackets.

Northern Hemisphere.

	1884.	1885.	1886.	1887.	1888.	1889.	1890.
Tacchini	50, 25*	45, 25*	45, 20*	35	35*, 15	40	45
Ricco and Mascari	55, 20*	55, 15*	45, 25*	30	35	45	45
Fényi	65, 45, 15*	45, 25*	45, 20*	45	40*, 20	43*, 25	45

Southern Hemisphere.

	1884.	1885.	1886.	1887.	1888.	1889.	1890.
Tacchini ..	(75), 25*, 5	25	35, 20*	45*, 25	45*, 25	45	45
Ricco and Mascari	(85), 25*, (5)	25	35	45*, 25	50*, 25*	45	50
Fényi	(75), 35*, 15*	(50), 35 10*	35*, 10*	50*, 30, 15	50*, 25	40	50*, 20

It will be seen that for these seven years, Fényi's results are in very close accordance with those deduced from the other two series of observations, thus generally endorsing those portions of the curves in Plates 6 and 7 covering this period.

It was mentioned in a previous paper† that the mean prominence curve for each hemisphere exhibited subsidiary maxima and minima. In the light of the present investigation, it is interesting to compare

* 'Publicationen des Haynald-Observatoriums, Kalocsa,' Heft VI, 1892, und VIII, 1902.

† 'Roy. Soc. Proc.,' vol. 71, p. 244.

this curve with that representing the changes of latitude of the zones of prominences. In every case, and for each hemisphere, the subsidiary maxima are coincident in time with the presence of two zones of prominences, each well-developed, while at the principal minima only one zone is in evidence.

We have already explained the fact that spots are restricted to a zone having its limits at latitudes $\pm 5°$ and $\pm 35°$, while prominences occur all over the sun's disc, even up to the poles, and also that spots always commence their cycle in high latitudes (about $\pm 35°$) and gradually approach the equator until within 5°, when a new cycle is commenced in high latitudes. Prominences on the other hand begin in comparatively low latitudes (about $\pm 24°$), and finish their cycle near the poles.

A glance at the Plates 6 and 7 brings out the interesting fact that at sunspot minima, when two zones of spots are in evidence, there is only one zone of prominences, while when only one zone of spots exists the prominences are for the most part confined to two zones.

The conclusions arrived at in the present communication may be summarised as follows:—

1. The centres of action of prominence activity undergo an apparently regular variation.

2. The direction of motion of these centres is from low to high latitudes, the reverse of that of spots, which travel from high to low latitudes.

3. At epochs of prominence minima (which are concurrent with sunspot minima) these centres of action are restricted to one zone (about latitude $\pm 44°$) in each hemisphere, while those of the spots occupy two zones in each hemisphere.

4. At nearly all other times these centres are apparent in two zones, while those of the spots occupy only one in each hemisphere.

5. The subsidiary maxima exhibited by the curves representing the percentage frequency of prominence activity for each entire hemisphere, are due to the presence of two well-developed centres of prominence activity in each hemisphere.

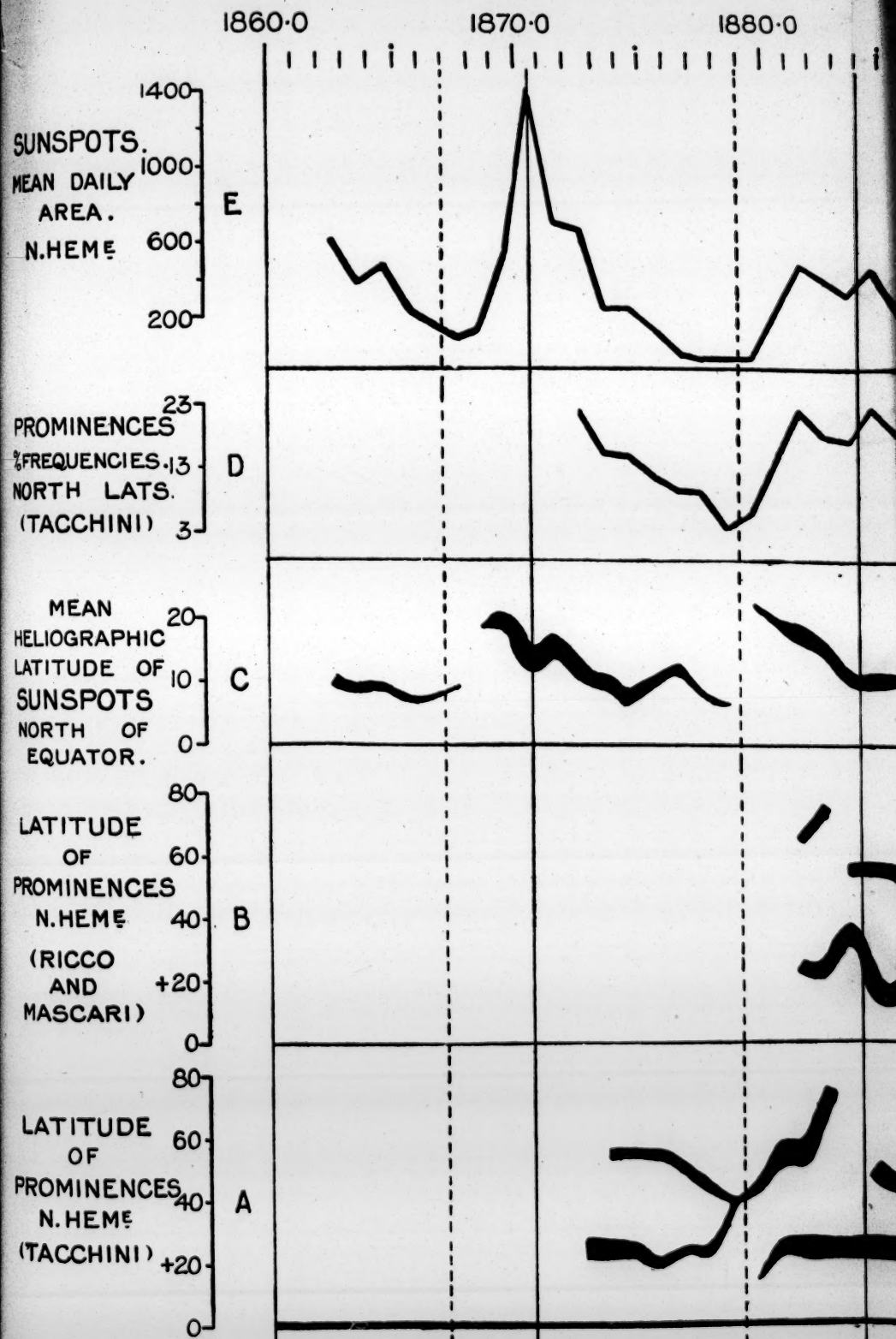


PLATE 6.—Curves showing the Relation between the Positions of the Centres of Action of Solar P
of Prominences (D), and Mean Daily Areas of Spots (E), for the No

Note.—The continuous and broken vertical lines represent the epochs of sunspot maxima and mi
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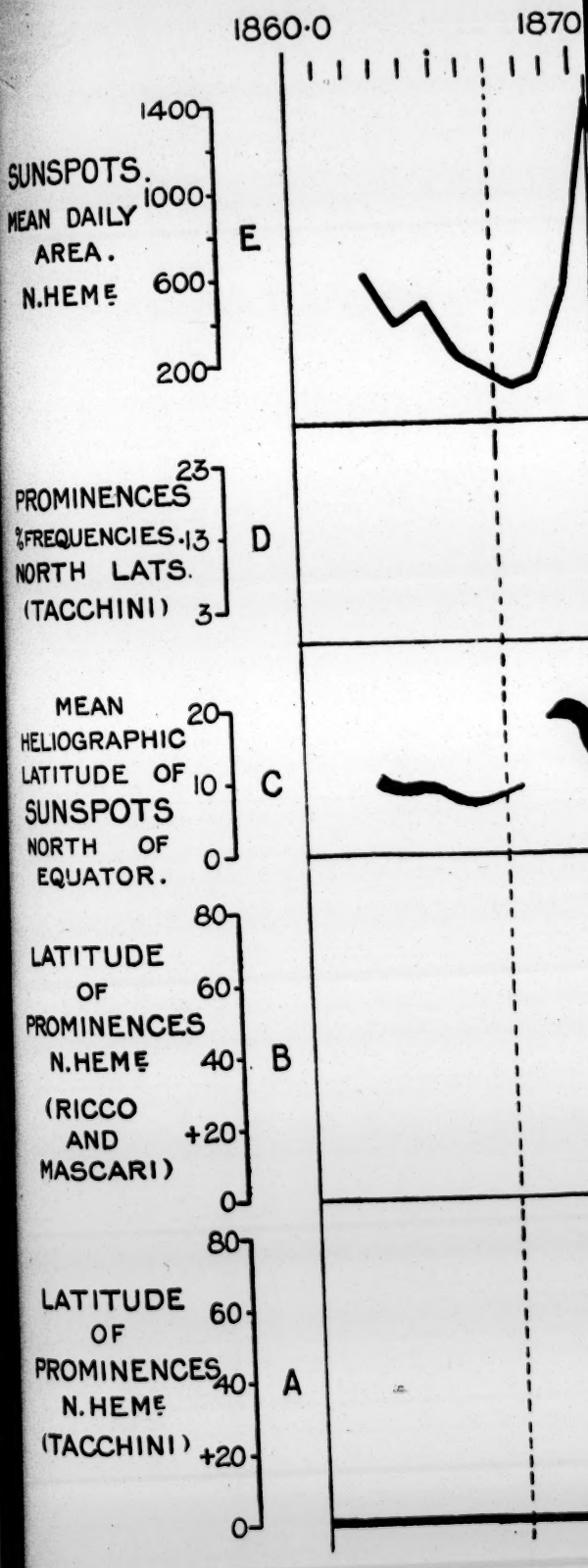
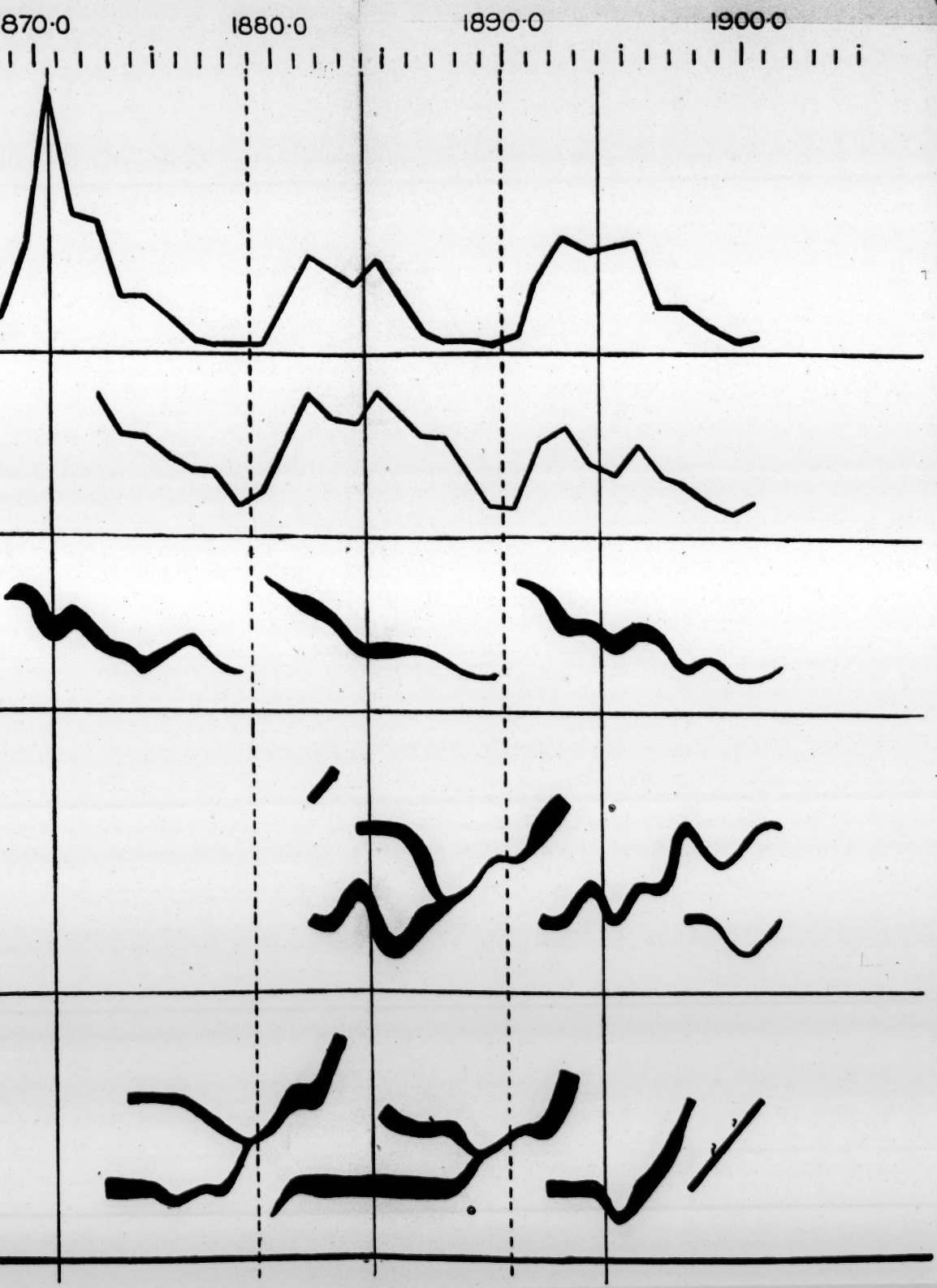


PLATE 6.—Curves showing the Relation between the
of Prominences (D), an

Note.—The continuous and broken vertical lines re



he Positions of the Centres of Action of Solar Prominences (A and B) and Spots (C), Percentage Frequency and Mean Daily Areas of Spots (E), for the Northern Hemisphere of the Sun.

represent the epochs of sunspot maxima and minima as determined from the mean daily areas of the whole solar disk.

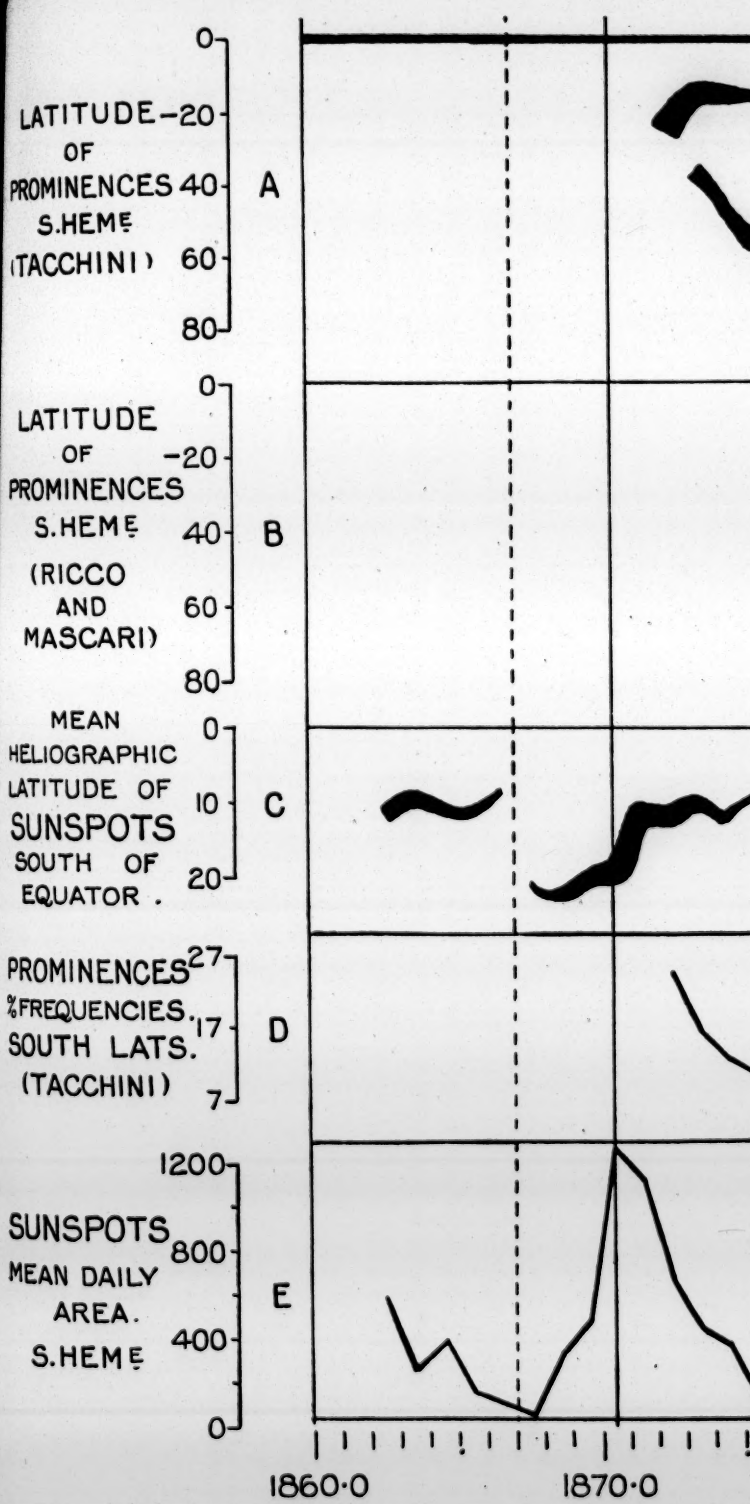
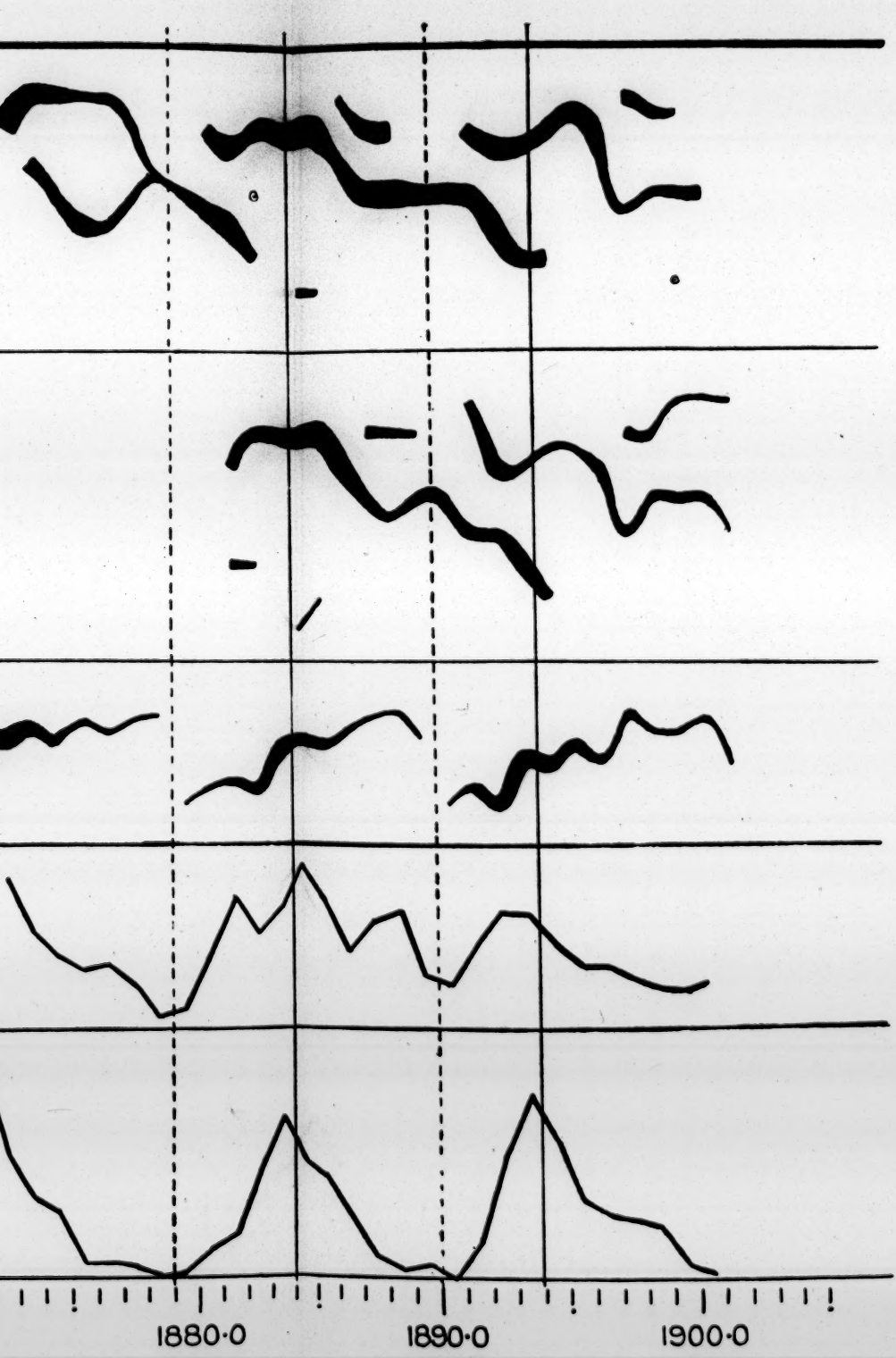


PLATE 7.—Similar Curves to those on Plate 1, only in this case the



case the Southern Hemisphere of the Sun is referred to. Vertical lines same as on Plate 1.

"On the Cytology of Apogamy and Apospory.—1. Preliminary Note on Apogamy." By J. B. FARMER, F.R.S., J. E. S. MOORE, and Miss L. DIGBY. Received March 24,—Read March 26, 1903.

The phenomena of apogamy and apospory have always been regarded as "short cuts" in the life history of ferns, and the fact that apparently either generation may be *directly* produced from the other, without the intervention of oosphere or spore respectively, has been taken to indicate that the gametophyte and sporophyte are homologous phases in the life histories of these plants. But since the cytology of the two generations has been carefully studied, it has become recognised that the prothallial generation is composed of cells, the nuclei of which possess only half the number of chromosomes that are characteristic of the alternate sporophyte generation.

In normal cases the doubling of the number of the chromosomes is effected during the transition of the gametophyte to sporophyte by the addition of the chromosomes belonging to the spermatozoid to those of the oosphere, and this double number is retained until they once more become reduced to one half in the formation of the spores that introduce again the gametophyte stage of the life-cycle.

It is obviously therefore of considerable theoretical interest to ascertain how the irregular transitions known as apospory and apogamy are effected. It is with the facts of apogamy that we are here concerned, the details relating to apospory being reserved for a future communication.

Certain species of nephrodium (*e.g.*, *N. pseudo-mas*, var. *polydactylum*) are known to produce prothallia on which the apogamous formation of sporophytes is of normal instead of rare occurrence.* By the kindness of Dr. Lang we received a number of prothallia in all stages of growth in which the special apogamous developments could be perfectly traced. An examination of them has resulted in the discovery of remarkable nuclear changes that appear to be obviously related to the apogamy of the prothallia in question.

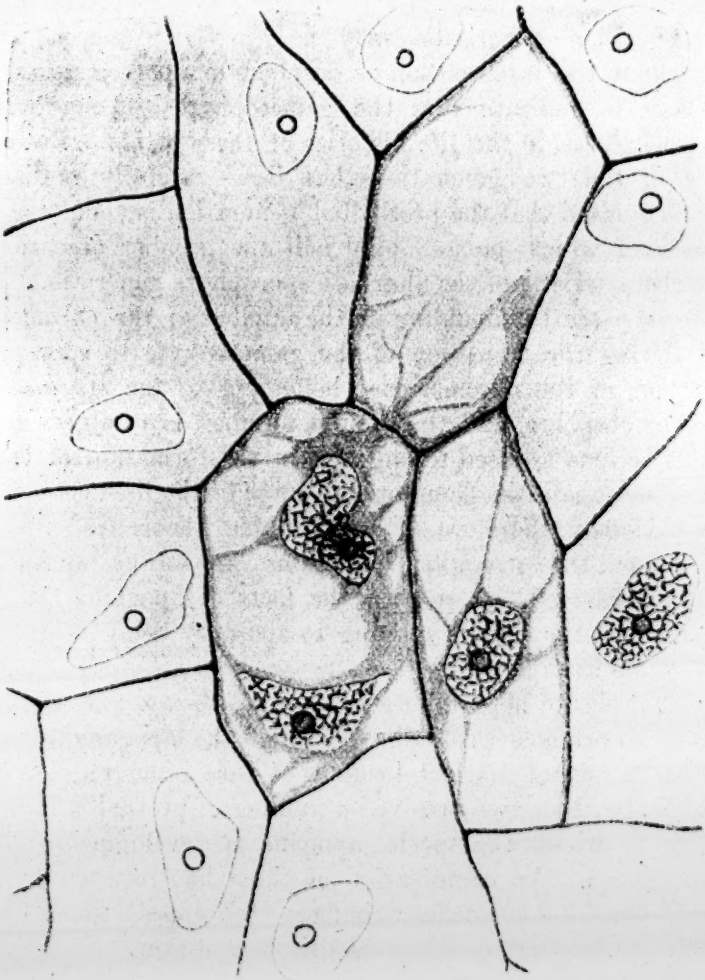
If very young prothallia are examined before any apogamous growths have begun to manifest themselves, it will be seen that cells not unfrequently occur in which two nuclei are present. This fact was recorded by Lang in the case of older prothallia, and was also figured by Heim† in the case of *Doodya caudata*, but he makes no mention of it in the body of his paper.

* Lang, "On Apogamy and the Development of Sporangia upon Fern Prothallia," 'Phil. Trans.,' series B, vol. 190, 1898, p. 214.

† Carl Heim, "Untersuchungen ü. Farn Prothallien," 'Flora,' vol. 82, 1896, p. 338, fig. 7.

Furthermore, it is to be observed that whenever this state of things is seen, there is always, so far as we have observed, at least one contiguous cell which is destitute of a nucleus (see figs. 1, 2, 4). We convinced ourselves of this highly important fact by examining entire prothallia that had been carefully stained, as there was always the

Fig. 1.



risk that in erections the appearance might be due to the displacement of the missing nucleus.

We were further able to trace the migration of the nucleus from one cell into that of its neighbour in a sufficient number of instances to convince us that this affords the explanation of the peculiar circumstances just mentioned (figs. 3, 4). In several instances the nucleus was seen in the act of passing through the wall, and in others the path through which it had traversed was plainly visible as a perfora-

FIG. 2.

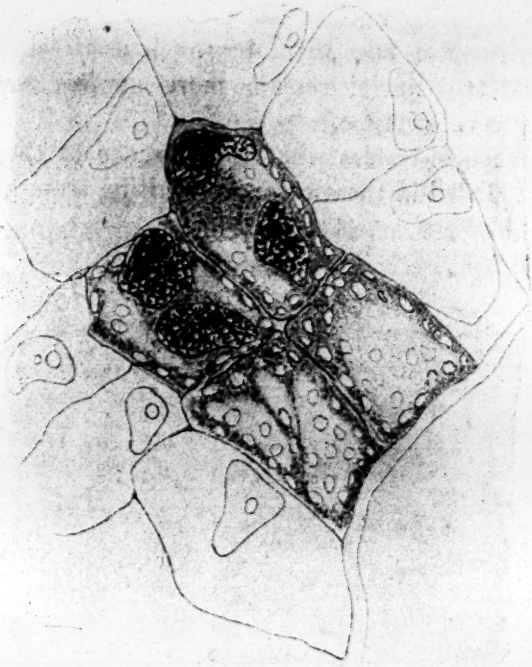
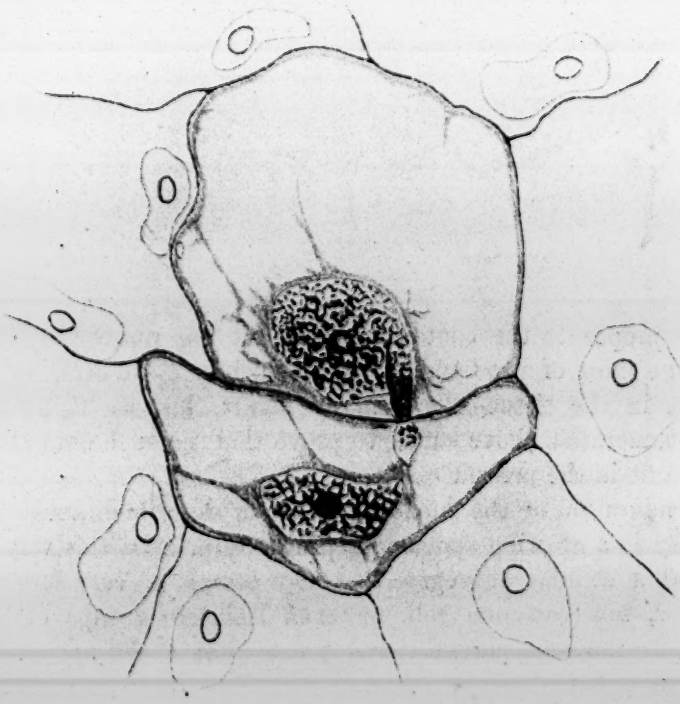


FIG. 3.

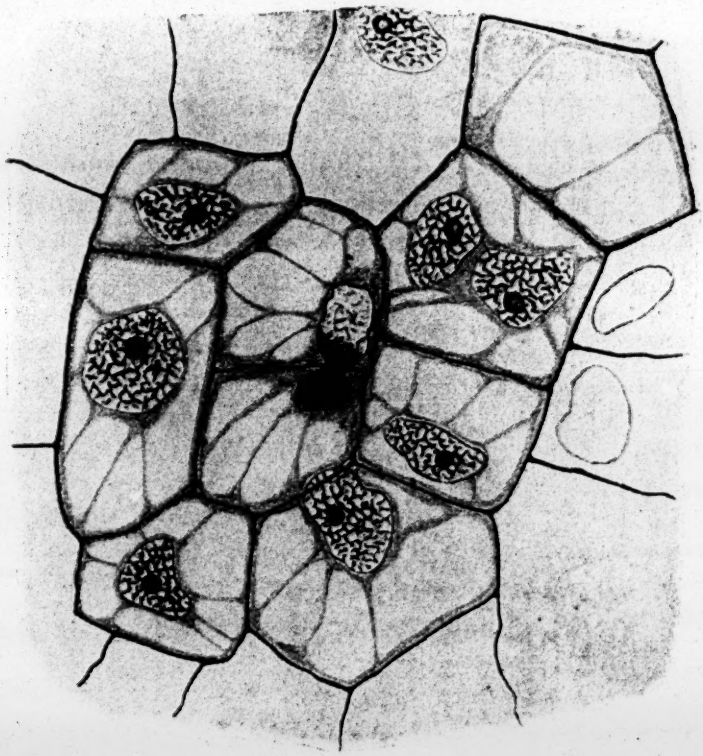


tion through which a strand of cytoplasm was still visible connecting the two cells.

When the migrating nucleus has passed into the neighbouring cell it sometimes fuses at once with the nucleus already present there, but often the two nuclei remain more or less separated for an appreciable interval of time.

It appears then to be clear that the presence of the pair of nuclei is not to be regarded in these cases as resulting from a division of the

Fig. 4.



nucleus proper to the containing cell, but has merely been arrested at a stage short of producing a wall. Such a state of things is very common in the tissue-cells of many plants, but we think that the facts enumerated above suffice to prove that no such simple explanation will fit in the present case.

The migration of the nucleus, as above described, goes on discontinuously in a growing apogamous prothalliun. In this way there is provided a cellular aggregate that may possess no very homogeneous character, nor can one cell, or even isolated groups of cells, be defined as the sole parent tissue from whence the apogamous outgrowth may have sprung. And this is quite in harmony with the

irregular growth of the new tissue, and with the sporadic appearance of sporophytic members, on the prothallium already described by Lang.

When the nuclei of the cells in the apogamous regions are examined in course of karyokinesis, they are seen to possess a much larger number of chromosomes than those of the ordinary tissue cells of the prothallium. Owing, however, to the manner in which the very numerous chromosomes are distributed on the spindle, an exact estimation of actual numbers is a task of considerable difficulty. There appear, however, to be forty and eighty in the respective classes of nuclei.

We regard the whole process as a kind of irregular fertilisation. The doubling of the chromosomes receives an explanation strictly analogous to that afforded by the normal fusion of oosphere and spermatozoid. But instead of one cell only (the oospore) serving as the starting point for the new generation, a number of such units loosely co-operate to produce it. And in this connection it is perhaps significant that the young plantlet is commonly borne on, and produced from, a special sporophytic outgrowth, of which the constituent cells may have become homologously differentiated into a sort of pro-embryo.

Instances are not wanting amongst lower animals to show that close cellular in-breeding may occur, and form part of the normal sexual series of events. It is not desired to press the analogy of such cases; but the case of Actinosphærium shows that a process indistinguishable from normal sexual fusion may occur between sister cells that have only lately arisen from the division of a parent cell.

We do not propose to enter on a full theoretical discussion of the bearings of these observations at the present time, but we hope to do so when we are in a position to deal completely with the corresponding cytological features associated with apospory.

"A Study of a Unicellular Green Alga, occurring in Polluted Water, with especial Reference to its Nitrogenous Metabolism." By HARRIETTE CHICK. Communicated by Professor RUBERT BOYCE, F.R.S. Received February 28,—Read March 12, 1903.

(From the Thompson Yates Laboratories, University College, Liverpool.)

[PLATE 8].

A small, unicellular, green alga was noticed to be frequently present in sewage, and sewage materials, when these had been kept for some time. The same alga was also found to have seeded itself in dilute ammoniacal solutions. This somewhat peculiar habitat seemed to promise that something of interest might repay a study of the physiology of the plant. On examination, under the microscope, it was found to be an extremely small, unicellular, non-motile alga, with a well-defined chromatophore, which latter contained a distinct pyrenoid. The general characteristics of the alga pointed to its being closely allied to *Chlorella* (Beyerinck),* and I have provisionally placed it in that genus. *Chlorella vulgaris* was originally described by Beyerinck as possessing either no pyrenoid, or an insignificant one; Chodat,† on the other hand, has described the species as having generally a pyrenoid, and has figured it accordingly. In a culture of *C. vulgaris*, obtained from Kral, I have been unable to detect a pyrenoid, and, since the alga here treated of possesses a very conspicuous one in its chloroplast, I have ventured to attach a new specific name, and to call the organism *Chlorella pyrenoidosa.* Though in the higher forms, such variation in cytological character between the species of a genus is practically unknown, yet the same difference occurs between different species of *Stichococcus*,‡ of *Raphidium*,§ and of *Pleurococcus*,‖ all genera of unicellular green algæ, showing very simple cell structure, and more or less nearly allied to *Chlorella*.

The peculiar natural habitat of the plant, and its appearance in ammoniacal solutions alluded to above, both pointed to a preference for ammonia in its nutritive fluid. In fact, in one instance, this alga was found to have grown in a dilute solution of ammonium chloride, and it was thought worth while to make an estimation of the ammonia. This was done by directly Nesslerising, and it was found that the ammonia had disappeared. By distilling the solution with alkali, however, the ammonia was obtained in a roughly quantitative yield.

* 'Bot. Zeit.,' 1890, p. 730.

† 'Beiträge z. Kryptogamenflora d. Schweiz.'

‡ Matruchot and Molliard, 'Rev. Gén. de Bot.,' vol. 13, 1902 (May).

§ Chodat, 'Mém. de l'Herb. Boiss.,' 1900, No. 17.

‖ Snow, 'Annals of Botany,' 1899; Chodat, 'Bull. de l'Herb. Boiss.,' 1894.

The algal material also grew exceedingly well when stroked upon the following ammoniacal medium:—2·0 grammes NH_4Cl, 1·0 gramme Na_2CO_3, 0·5 gramme K_2HPO_4, 15·0 grammes agar-agar, in 1 litre water. The growth was, however, contaminated with numerous bacteria.

This evident ready assimilation of ammonia pointed to the possibility of the organism's playing some part in the "purification" of sewage or further natural "purification" of sewage effluents, and its nitrogenous metabolism was studied with this view.

Mode of Preparation of a Pure Culture.

The first step necessary towards a study of the chemical physiology of the alga was the preparation of a pure culture. As the separation of algæ in pure culture is not of very common occurrence, few as yet having been obtained, it may be permissible to describe in detail the method adopted. The following methods were tried without success. Sterilisation, as regards bacteria, was attempted by exposure of liquid cultures to sunlight, and hydrogen peroxide was also tried as a means of killing the adherent bacteria, but the alga appeared to be susceptible also. More purely bacteriological methods were employed, viz.:—by stroking in great dilution over the surface of potatoes, and by "pouring" plates of ammonium agar, or ammonium gelatine, and incubating in sunlight, at a low temperature. All these methods failed for one reason or another, but the organism was finally separated in the following way. It seemed advisable to employ an ammonium-containing medium, and ammonium agar was therefore selected. Plates of this medium were poured and allowed to set. An ammoniacal solution was also made and sterilised, having the following composition:—0·5 gramme Na_2CO_3, 0·5 gramme K_2HPO_4, 0·1 gramme NH_4Cl, 1 litre tap-water or distilled water. This solution will in future be alluded to as "solution A."

A few drops were allowed to drop upon the surface of the poured plates, then a small portion of the impure material was added to the first plate, and the whole was brushed over the surface. The same brush was then used to brush the liquid over the second plate, and so on, to six plates or more, without the addition of fresh algal material, so that a considerable dilution was obtained. The plates were kept in the light, in as much sunshine as was possible, protected from dust, and in a damp atmosphere. In seven to fourteen days, green growth was generally visible, and after three or four weeks, definite algæ-colonies were to be distinguished among the colonies of bacteria.

In this way pure cultures of *Chlorella pyrenoidosa* were obtained from

(1) Material of Ducat Filter Bed (Hendon).
(2) Sludge wash-water (Leeds).

This method may be relied upon to yield pure cultures, for it has since been repeated, and again proved successful.

The following is a fairly comprehensive diagnosis of *Chlorella pyrenoidosa.*

Chlorella (Beyerinck)* very minute, unicellular, non-motile, spherical or elliptical green algæ; chromatophore single, parietal, with or without a pyrenoid; cells isolated, showing no tendency to form colonies; multiplication by division of the cell-contents to form a number of daughter cells.

C. pyrenoidosa (sp. n.):—cells spherical, 3—5 μ in diameter, sometimes attaining 11 μ; chromatophore single, parietal mantle-shaped, covering nearly the whole of the cell-wall; pyrenoid conspicuous and single (see Plate 8, fig 3); reproduction by successive division of the cell-contents to form within the mother cell as many as eight daughter cells, which subsequently become free of the mother cell wall (see fig. 5).

Physiological characters.—Showing a marked preference for ammonia and ammoniacal compounds (*e.g.*, urea) compared with nitrates in its culture fluids; growth and multiplication largely increased by the addition of glucose to the culture fluids, causing the cells and cell-contents to assume a changed and characteristic appearance.

Chlorella pyrenoidosa resembles *C. protothecoides* (Krugert) very closely, both in its physiology and morphology; it differs, however, in the possession of a pyrenoid. This pyrenoid with few exceptions is very conspicuous, and can often be stained blue with iodine, being the only part of the cell which shows this staining. *C. pyrenoidosa* differs also from *C. vulgaris* (Beyerinck)‡ in many of its physiological properties, as well as by the possession of the well-marked pyrenoid. Its morphology bears, however, a very close resemblance to that of a green alga isolated and studied by Kossowitsch.§ This alga itself resembled *C. vulgaris* (Beyerinck) and *Cystococcus* (Nägeli)|| very closely, and Kossowitsch decided to call it "Cystococcus," in spite of certain small differences which existed. It has seemed to me the wisest plan to attach the alga, diagnosed above, to the genus *Chlorella*, and to add a new specific name "pyrenoidosa," although it resembles the *Cystococcus* figured and described by Nägeli, and it seems not unlikely that it may be identical with Kossowitsch's *Cystococcus.*

C. pyrenoidosa grows exceedingly well, in pure culture, upon nutrient gelatine, nutrient agar or ammonium agar, and also in many liquid media. In media containing glucose the growth is much

* 'Bot. Zeit.,' 1890, p. 725; 'Cent. f. Bakt.,' vol. 13, 1893, p. 368.

† Zopf's 'Beiträge z. Morph. u. Phys. Nied. Org.,' Leipzig, vol. 4, 1894, p. 92.

‡ 'Bot. Zeit.,' 1890, p. 725; 'Cent. f. Bakt.,' vol. 13, 1893, p. 368.

§ 'Bot. Zeit.,' 1894, vol. 6, p. 97.

|| 'Gattunger einzelliger Algen,' Zürich, 1894, p. 84.

stimulated, and this is particularly rapid when grown upon nutrient agar or gelatine to which glucose has been added. The liquid medium *par excellence* for studying this organism, is sterilised sewage, in which it preserves its normal form and characteristics. In solution A, it also grows well, while if to the liquid a small quantity of glucose is added, the growth is much stimulated, and the individuals assume the appearance, described below, characteristic of growth in glucose media.

Accompanying the much more abundant development, the nitrogen assimilation is also increased, as will be seen later.

Glucose-containing media (fig. 4). When grown upon media containing glucose, while the general growth is much improved, and the individual cells are also larger in size upon the whole, the green colour of the cell-contents is much paler, and sometimes almost disappears, while the chromatophore is much disorganised. The contents of the cell appear to be segregated into a variable number of slightly refractile bodies, usually pale greenish-yellow in colour, which are apparently free in the cell. If when mounted in water under the microscope, a slight pressure is applied to the coverslip, the cell envelope is easily ruptured, and these bodies are liberated, and float freely in the surrounding liquid. The protoplasm also, in the case of glucose cultures, shows small granules, and the pyrenoid is no longer to be traced, but a certain amount of staining with iodine may be noticed in the granular protoplasm. These changes closely resemble those noticed respectively by Krüger* in the case of his *Chlorothecium saccharophilum* and *Chlorella protothecoides*, and by Matruchot and Molliard† in the case of *Stichococcus bacillaris*, when these algæ were grown upon glucose containing media.

The results of growing *C. pyrenoidosa* upon media containing lactose or saccharose also resembled those of the above authors. These two sugars appear to possess a far lower nutritive value than does glucose, while the algal cells preserve their normal appearance. When grown upon ammonium agar, the growth is comparatively slow, the individuals appear larger in size than when grown upon ordinary nutrient agar, their green is darker and the cell-contents are of the normal type.

Quantitative Chemical Experiments.

The evident preference of this organism for ammoniacal culture solutions seemed to be a reason for studying its nitrogen assimilation. The absorption of ammonia by algæ is not a new discovery; it is well known, from the researches of Letts and Hawthorne,‡ that *Ulva latissima*

* Zopf's 'Beiträge z. Morph. u. Phys. Nied. Org.,' Leipzig, vol. 4, 1894, p. 91.

† 'Rev. Gén. de Bot.,' vol. 13, 1902.

‡ Letts and Hawthorne, 'Roy. Soc. Proc., Edin.,' 1901, p. 268; Letts, "Report on the Scheme of Sewage Purification proposed for Belfast, and its Probable Effects on the Lough."

absorbs ammonia with extreme rapidity, and, consequently, will grow and develop to an extraordinary extent in sewage-polluted water, for such water contains a comparatively large amount of ammonia. *C. pyrenoidosa* also absorbs ammonia in a marked degree, and this fact lends practical importance to the study of its nitrogenous metabolism, for it also may have some bearing upon the purification of sewage. Although the organism is itself very small, and the absolute quantities it tackles also insignificant, yet the results of the following experiments, themselves upon a very small scale, tend to show that if *C. pyrenoidosa* were growing in great quantities (*e.g.*, in the bed of a river polluted with sewage), very important changes might be effected in the composition of the water in which it grew.

The method of pure cultures was adopted in every case, unless otherwise stated. "Pasteur flasks" were used to contain the cultures; these were sterilised and then filled with the sterilised culture fluid by means of sterilised pipettes. One flask was always kept sterile, as a control, while a second was inoculated with *Chlorella pyrenoidosa*. The flasks were kept, often for months, in a sunny place, protected from dust, and in a damp atmosphere in order to prevent evaporation. These Pasteur flasks can be obtained to hold 300 c.c., and this size, when containing 150 c.c. of fluid, was found to be extremely convenient. To see if the cultures and controls remained in a sterile condition as regards bacteria, they were tested from time to time by abstracting a few drops and adding them to tubes of ordinary bouillon. These tubes were incubated both at the ordinary temperature and at blood heat, and were watched to see if any bacterial growth took place. The risk of contamination was found to be extremely small, and its occurrence very rare. The figures in the following tables refer in every case to cultures that were successfully maintained in a state of purity. It was found, in a few cases, as will be seen in the following tables, that difficulty was experienced in maintaining constant the analysis of the sterile control, when the experiment lasted over a long period. In these cases it would seem as though the ammonia present suffered a slight volatilisation, but this does not vitiate the analysis of the corresponding cultures, because the changes in the control are, in all cases, insignificant when compared with the changes taking place in the inoculated fluid. In such cases, it seems a fair procedure to subtract the change in the control from that in the culture, and to consider the difference as due to the activity of the alga.

Methods of Analysis.—Samples for analysis were removed from the flasks under examination from time to time by means of sterile pipettes. These pipettes were always carefully cleaned, plugged, and then sterilised by heating in a hot-air steriliser at 150° C. for one hour. To protect them from any dust they might attract during this

operation, their ends were fixed by means of cotton wool in the mouths of test tubes, and the whole wrapped in filter paper, and left so until actually used. When the samples were removed for analysis, the liquid was at the same time tested for sterility in the way already described.

The culture liquids employed were sterilised sewage and sewage effluents, and also sterilised artificial liquids whose composition was arranged to resemble that of sewage as nearly as possible. The sewage was usually allowed to stand for several days, then filtered, and afterwards sterilised by successive heatings at 100° C. The artificial culture media were the solution, A, referred to above, and also the same solution to which a small amount of glucose has been added (about 0·25 per cent.).

The methods of analysis were those usually adopted in water and sewage analysis. In the estimation of the free ammonia a small portion of the liquid to be analysed (3 to 10 c.c.) was diluted with about 500 c.c. ammonia free water and distilled. Three successive portions of 50 c.c. were distilled off, and the ammonia contained in them estimated by means of adding Nessler's solution and comparing the yellow tint with that given by definite amounts of standard ammonium chloride solution. "Albuminoid ammonia" was afterwards estimated by adding a fixed amount of "alkaline permanganate" solution,* distilling as long as ammonia came over in the distillate, and estimating these amounts in the same way. It is possible that in some cases the quantities of albuminoid ammonia given in the analyses in the following tables may be slightly too low. For, in certain instances, towards the end of the distillation, the ammonia came over, in the distillate, very slowly and in very small quantities. It was considered more accurate in these cases to discontinue the analysis when these amounts fell below a certain very small value, since the error in estimating such very small quantities becomes comparatively great.

The presence of nitrates or nitrites was detected by means of diphenylamine-sulphuric acid and metaphenylene-diamine, while quantitative estimations were made by means of the "copper-zinc couple" method in which the nitrogen present as nitrates or nitrites is estimated as ammonia.† To allow for any ammonia that might have been originally present in the solution or introduced as traces during the analysis, a control estimation was always made. This analysis was carried out in every way like the real one, except that no "couple" was introduced, and the ammonia thus obtained was subtracted from that found in the actual estimation.

The analysis of the cultures of this alga by these methods is by no

* 'Volumetric Analysis,' Sutton, 8th ed., p. 512.

† *Ibid.*, p. 482.

means easy, for the quantities treated are exceedingly small, the methods of analysis adopted demand a comparatively large margin of experimental error, and the length of time, over which many of the experiments must continue, is also a drawback. The sterilisation of flasks and pipettes must also introduce a slight error where such small quantities of ammonia are concerned. The estimation of the albuminoid ammonia is frequently rendered troublesome by the tendency of the liquid to bump while distilling. It has been found that this difficulty is usually obviated if the ammonia-free water used for dilution is well aërated, and if, from time to time, small quantities of fresh aërated, ammonia-free water are added during the distillation.

The absorption of ammonia, as this alga grows in ammonia-containing culture liquids, is well shown by the results of the analyses in Tables I—VII. At the same time, the amount of albuminoid ammonia present steadily increases, and, on the whole, a fairly even balance is maintained, which can be seen by comparing the quantities of total nitrogen.

The evident preference of *Chlorella pyrenoidosa* for its nitrogen as ammonia, rather than in an oxidised form, is seen from Tables II, III, IV. In Table III an impure culture of the alga was allowed to grow alongside the pure culture, and it will be seen that similar changes had been taking place in the former, although they were not so well marked. In the case of the pure culture, nearly all the free ammonia was absorbed in six weeks, a considerable amount of albuminoid ammonia was produced, while the nitrates and nitrites appear to have remained free from any attack on the part of the alga. In the case of the impure culture less free ammonia was absorbed, less albuminoid ammonia formed, and some of the oxidised nitrogen present in the solution had been assimilated.

The presence of a small proportion of glucose (0·25 per cent.) has a very remarkable effect in stimulating the nitrogen assimilation of the plant (see Tables VI and VII). In these two cases, cultures were started in exactly similar solutions, except that to the one set the above small amount of pure glucose was added. In this case (Tables VI *b* and VII *b*), practically all the free ammonia was absorbed, and largely converted into albuminoid ammonia in an extremely short time, when compared with the cultures grown in the solutions containing no sugar. The glucose cultures showed evidence of much more growth, and the individual algal cells were also much paler in colour.

It appeared to be a point of interest whether this albuminoid ammonia, so invariably formed in these cultures of *Chlorella pyrenoidosa*, corresponded to nitrogenous substances formed within the plant cell and kept there, or whether such substances were formed, and then

allowed to go free in the liquid. With the object of settling this point, cultures which had manufactured a good deal of albuminoid ammonia, were subjected to centrifugalisation, and analyses made of the clear solution (see Tables VI *b* (analysis No. 3) and V). The clear solution in either case contained no more albuminoid ammonia than the control, proving that the elaborated nitrogenous substances were entirely retained within the cell body.

In two cases, however (Tables VI *b* (analysis No. 4) and VII *b*), older cultures were examined. The word "older" is here used to imply that these cultures had passed through many more cell-generations, judging from the appearance of the growth and the size of deposit present. In these cases, a considerable amount of albuminoid ammonia was found to exist in the clear liquids. This seemed to prove that the algal individuals, under certain conditions, yield up to their culture liquid somewhat complicated nitrogenous matters in a soluble form, and a probable explanation would be that they exhibit this phenomenon when the cells are in a dying or dead condition. This fact assumes a certain significance when it is remembered that the natural habitat of this alga is ammoniacal liquids, hence notably polluted waters. The actual amount of assimilation and excretion is extremely small, and it would be difficult to assert that the presence and growth of such an alga as *Chlorella pyrenoidosa* was of very great importance in nature. But the effect of this alga, if present, would probably be to leave the water, in which it has grown, in what would be termed a "more impure" condition. It is evident from the above experiments that the organism possesses the faculty of converting saline ammonia into albuminoid ammonia, which, under certain conditions, is discharged from the cells into the culture fluid. "Albuminoid ammonia" is a name given to a certain class of substances on account of facts connected with their analysis; these substances are quite unstudied, and, in this instance, may be perfectly harmless compounds. But the presence of albuminoid ammonia to more than a very small degree has always been considered, perhaps without sufficient foundation, to show evidence of dangerous pollution, and hence to be most prejudicial to any water. The formation and excretion of such compounds by a green alga appears to be a new phenomenon, and is not without especial interest in this connection.

An attempt was made to compare the nutritive value for *Chlorella pyrenoidosa* of various nitrogenous substances, and with this object a series of nutrient fluids were prepared of the same composition as those used by Krüger* in his experiments with *Chlorella protothecoides* and *Chlorothecium saccharophilum*.

* Krüger, Zopf's 'Beiträge z. Phys. u. Morph. Nied. Org.,' Leipzig, vol. 4, p. 103.

A stock solution was made having the following composition:—

Glucose 1·0 per cent., K_2HPO_4 0·2 per cent., $MgSO_4$ 0·04 per cent., $CaCl_2$ 0·02 per cent., distilled water 100 c.c.

With this stock solution eight different culture solutions were prepared containing respectively 0·1 per cent. of the following:—

1. Asparagine.
2. Aspartic acid.
3. Urea.
4. Uric acid.
5. Peptone.
6. Xanthin.
7. Hippuric acid.
8. Stock solution, without further addition.

Of these solutions, the cultures grown in those containing urea and uric acid were found to flourish exceedingly well, and by far the best, while of the others, the culture in the liquid containing xanthin gave evidence also of being very well nourished.

Uric Acid Culture.—An analysis was made of this culture in the ordinary way. In the case of the control liquid, it was found that no free ammonia was obtained, and comparatively very little of the nitrogen of the uric acid was yielded as albuminoid ammonia. It was found, however, that when *Chlorella pyrenoidosa* had been growing in the liquid, the yield of albuminoid ammonia was much increased, showing that much of the nitrogen of the uric acid had been assimilated by the organism and converted into albuminoid ammonia.

Urea Culture.—In the case of the control urea-containing liquid, about 25 per cent. of its nitrogen was yielded, on analysis, as free ammonia. In the case of the inoculated fluid, about 30 per cent. of this ammonia was found to have disappeared, that is, to have been assimilated. After addition of alkaline permanganate, distinctly more ammonia was yielded from the liquid in which alga had grown than from the sterile control.

It is difficult to draw very positive quantitative conclusions from the results of the above two experiments, but it seems evident at least that this alga is able to assimilate with very great ease and without any previous decomposition the nitrogen both of urea and uric acid. The former substance has also been found by other observers* to be a useful source of nitrogen for plants in certain instances.

In the case of *Chlorella pyrenoidosa*, however, these facts are of special interest, when it is remembered that the plant naturally grows in water that has been contaminated by sewage.

* Cameron, 'Trans. Brit. Assoc.,' 1857; Ville, 'Compt. Rend.,' vol. 55, p. 32.

Table I.—Showing Assimilation of Ammonia.

Culture in Solution "A." Started October 21, 1901.

No. of analysis.	Date of analysis.	—	Control.	Culture of *Chlorella pyrenoidosa*.
			Per cent. nitrogen.	Per cent. nitrogen.
1	21.10.1901	Free ammonia	0·00209*	—
2	26.10.1901	Free ammonia	0·00183*	0·00144*
3	2.11.1901	Free ammonia	0·00152*	0·00114*
4	28.11.1901	Free ammonia	0·00194†	0·00071†
		Albuminoid ammonia	0·00062	0·00081
		Nitrites	0	0
		Nitrates	0	0

* Estimated by direct Nesslerisation.

† Estimated, after distilling, by Nesslerisation.

Table II.—Showing the Assimilation of Ammoniacal Nitrogen in preference to Oxidised Nitrogen.

Culture in a Sterilised Sewage Effluent (Land).

Date of analysis.	—	Control.	Culture.
		Per cent. nitrogen.	Per cent. nitrogen.
31.10.1901	Free ammonia	0·000142	—
	Albuminoid ammonia	trace	—
7.12.1901	Free ammonia	0·00009	0
	Albuminoid ammonia	0·00007	0·00025
	Nitrates	0·00215	0·00213
	Nitrites	0	Faint trace
	Nitrogen— Total (by addition)	0·00231	0·00238

Table III.—Showing the Assimilation of Ammoniacal Nitrogen in Preference to Oxidised Nitrogen, and Formation of Albuminoid Ammonia.

Culture in Sterilised Sewage, which had stood for some time. Started November 8, 1901.

Date of analysis.	—	Control.	Pure culture of *Chlorella pyrenoidosa.*	Culture of *Chlor. pyrenoidosa* contaminated with the bacteria which usually accompany it.
		Per cent. nitrogen.	Per cent. nitrogen.	Per cent. nitrogen.
8.11.1901	Free ammonia	0·00124	—	—
	Albuminoid ammonia	0·00023	—	—
18.12.1901, to 20.12.1901	Free ammonia	0·00141	0·00030	0·00058
	Albuminoid ammonia	0·00025	0·00105	0·00097
	Nitrites	Small reaction.	Small reaction	Small amount
	Nitrites and nitrates	0·00142*	0·00135†	0·00119
	Total nitrogen (by addition)	0·00308	0·00270	0·00274

* Free ammonia was got rid of by boiling before the addition of the ZnCu couple.

† Probably under estimated.

Table IV.—Showing the Assimilation of Ammoniacal Nitrogen in Preference to Oxidised Nitrogen, and the Formation of Albuminoid Ammonia.

Culture in Sterilised Sewage. Started October 31, 1901.

No. of analysis.	Date of analysis.	—	Control.	Pure culture of *Chlorella pyrenoidosa*.
			Per cent. nitrogen.	Per cent. nitrogen.
1	31.10.1901	Free ammonia	0·00247	—
		Albuminoid ammonia	0·00041	—
2	20.10.1901 and 21.10.1901	Free ammonia	0·00231	0·00237
		Albuminoid ammonia	0·00072	0·00059
3	20.12.1902 21.12.1902 and 22.12.1902	Free ammonia	0·00276	0·00082
		Albuminoid ammonia	0·00063	0·00230
		Nitrites	Small reaction	Small reaction, but greater.
		Nitrites and nitrates	0·00128	0·00131
		Total nitrogen (by addition)	0·00467	0·00443

Table V.—Showing the Assimilation of Ammoniacal Nitrogen, Formation of Albuminoid Nitrogen, and the Results of Centrifugalisation Experiments.

Culture in Sterilised Sewage, containing no Oxidised Nitrogen. Started August 4, 1902.

No. of analysis.	Date of analysis.	—	Control.	Culture of *Chlorella pyrenoidosa.*	
				Whole culture.	Clear solution after centrifugalisation.
			Per cent. nitrogen.	Per cent. nitrogen.	Per cent. nitrogen.
1	4.8.1902	Free ammonia	0·00251		
		Albuminoid ammonia	0·00033		
		Nitrites and nitrates	0·00	—	—
		Total nitrogen (by addition)	0·00284		
2	9.10.1902	Free ammonia	0·00258	0·00145	
		Albuminoid ammonia	0·00032	0·00068*	
		Nitrites and nitrates	0·00	0·00	—
		Total nitrogen	0·00290	0·00213	
3	14.10.1902 and 15.10.1902	Free ammonia	0·00233	0·00131	0·00115
		Albuminoid ammonia	0·00029	0·00108	0·00041
		Nitrites and nitrates	0·00	0·00	0·00
		Total nitrogen	0·00262	0·00239	0·00156
4	4.11.1902	Free ammonia	0·00212	0·00049	0·00041
5	18.12.1902	Free ammonia		0·00013	0·00012
		Albuminoid ammonia		0·00182	0·00058
		Nitrites and nitrates	—	0·00	0·00
		Total nitrogen		0·00194	0·00070

* Probably under estimated.

Tables VI (*a*) and (*b*).—Showing increased Nitrogen Assimilation when Glucose is present in the Culture Fluid, and the Result of Centrifugalisation Experiments, in the latter instance, both in the case of Younger and Older Cultures.

Table VI (*a*).—Culture in Solution "A." Started November 3, 1903.

No. of analysis.	Date of analysis.	—	Control.	Culture of *Chlorella pyrenoidosa*.
			Per cent. nitrogen.	Per cent. nitrogen.
1	3.11.1902	Free ammonia	0·00216	
		Albuminoid ammonia	0·00042	
		Nitrites and nitrates	0·00	—
		Total nitrogen (by addition)	0·00258	
2	21.11.1902 and 22.11.1902	Free ammonia	0·00231	0·00202
		Albuminoid ammonia	0·00040	0·00039
		Nitrites and nitrates	0·00	0·00
		Total nitrogen	0·00271	0·00241

Table VI (*b*).—Culture in Solution "A," to which a small amount of Glucose had been added (about 0·25 per cent.). Started November 3, 1902.

No. of analysis.	Date of analysis.	—	Control.	Culture of *Chlorella pyrenoidosa.*	
				Whole culture.	Clear solution after centrifugalisation.
			Per cent. nitrogen.	Per cent. nitrogen.	Per cent. nitrogen.
1	3.11.1902	Free ammonia	0·00241		
		Albuminoid ammonia	0·00045		
		Nitrites and nitrates	0·00	—	—
		Total nitrogen (by addition)	0·00286		
2	21.11.1902	Free ammonia	Was unfortunately contaminated between 3.11.1902 and 21.11.1902.	0·00046	
		Albuminoid ammonia		0·00122	
		Nitrites and nitrates		0·00	—
		Total nitrogen (by addition)		0·00168	
3	24.11.1902 and 25.11.1902	Free ammonia		0·00021	0·00025
		Albuminoid ammonia		0·00152	0·00025
		Nitrites and nitrates		0·00	0·00
		Total nitrogen		0·00173	0·00050
4	22.12.1902	Free ammonia		0·00034	0·00032
		Albuminoid ammonia		0·00160	0·00058
		Nitrites and nitrates		0·00	0·00
		Total nitrogen		0·00194	0·00090

Tables VII (*a*) and (*b*).—Showing increased Nitrogen Assimilation when Glucose is present in the Culture Liquid, and the Result of Centrifugalisation Experiments in the latter case.

Table VII (*a*).—Culture in Solution "A." Started August 5, 1902.

No. of analysis.	Date of analysis.	—	Control.	Culture of *Chlorella pyrenoidosa*.
			Per cent. nitrogen.	Per cent. nitrogen.
1	5.8.1902.	Free ammonia	0·00268	
		Albuminoid ammonia	0·00012 (?)	
		Nitrites and nitrates	0·00	—
		Total nitrogen (by addition)	0·00280	
2	19.11.1902.	Free ammonia	0·00221	0·00166
		Albuminoid ammonia	0·00039	0·00091
		Nitrites and nitrates	0·00	0·00
		Total nitrogen	0·00260	0·00257

Table VII (*b*).—Culture in Solution "A," to which a small amount of Glucose had been added (about 0·25 per cent.). Started August 5, 1902.

No. of analysis.	Date of analysis.	—	Control.	Culture of *Chlorella pyrenoidosa.*	
				Whole culture.	Clear liquid after centrifugalisation.
			Per cent. nitrogen.	Per cent. nitrogen.	Per cent. nitrogen.
1	5.8.1902	Free ammonia	0·00254		
		Albuminoid ammonia	0·00015 (?)	—	—
		Nitrites and nitrates	0·00		
		Total nitrogen	0·00269		
2	23.10.1902	Free ammonia	0·00231	0·00070	
		Albuminoid ammonia	0·00022	0·00141	
		Nitrites and nitrates	0·00	0·00	—
		Total nitrogen	0·00253	0·00211	
3	20.10.1902	Free ammonia	0·00210	0·00074	0·00063
		Albuminoid ammonia	0·00034	0·00166	0·00081
		Nitrites and nitrates	0·00	0·00	0·00
		Total nitrogen	0·00244	0·00240	0·00144

General Conclusions.

It appears to be generally true that most plants containing chlorophyll prefer the nitrogen of their food in the form of nitrates. On the other hand, many observers have shown that certain plants can assimilate nitrogen in the form of ammonia, and in fact prefer it. Of these latter experiments, many were not performed with the precautions necessary to prevent the access of bacteria (*e.g.*, nitrifying organisms), and hence must be considered inconclusive. Some, on the contrary, were most carefully carried out with every precaution, and among these may be mentioned those of Krüger,* who showed that his *Chlorella protothecoides* and *Chlorothecium saccharophilum* (two algæ nearly allied to *Chlorella pyrenoidosa*) could both assimilate their nitrogen

* Zopf's 'Beiträge z. Phys. u. Morph. Nied. Org.,' Leipzig, 1894, vol. 4, p. 115.

when offered in the form of ammonium salts, while the latter alone could assimilate nitrate nitrogen. Artari,* working with the gonidia of two lichens, isolated in pure culture, showed that, after peptone, asparagine and ammonium sulphate were the useful sources of nitrogen for these algæ, and other observers† have made equally careful experiments in the case of some of the higher plants, and have shown that not only simple ammonium salts, but also compound ammonias, such as methylamine, ethylamine, can readily be assimilated.

From a physiological point of view *Chlorella pyrenoidosa* must be included in this second class, for it has been shown, as the result of quantitative as well as qualitative experiments, that this alga prefers its nitrogen to be presented to it in the form of ammonia or ammoniacal compounds. Among the latter urea, uric acid, &c., rank high in nutritive value.

It would also appear from the foregoing chemical experiments that this ammonia, after being absorbed by the cell, is elaborated into albuminoid ammonia,‡ a term used to describe certain nitrogenous bodies of ammoniacal nature, which yield ammonia when boiled with alkaline permanganate of potash; in fact, almost all the nitrogen assimilated would appear to remain in this comparatively simple form. This nitrogen, for example, is more easy of attack than that contained in uric acid. These elaborated nitrogenous compounds appear to be retained wholly within the cell body, but under certain conditions, only observed in the case of "older" cultures, they seem to escape from the cell, and can be traced free in the liquid.

The presence of glucose in a culture liquid frees the alga from the necessity it would otherwise experience of manufacturing carbohydrate for itself. The algal cell, being thus relieved of a certain part of its ordinary work, appears to be enabled to reproduce itself much faster, and its nitrogen assimilation is also much increased, though, owing to the increased multiplication of cells, it would be impossible to say that the nitrogen assimilation per cell was increased. The chlorophyll body of the cell, at the same time, gives evidence that its function has been interfered with by a most striking change in form and in the amount of chlorophyll. It is distinctly noteworthy that neither cane-sugar nor lactose can be substituted for glucose in this connection.

The effect of glucose in causing a definite change in the chlorophyll body, and in generally stimulating growth, would not appear to be an isolated fact. It has been shown by other observers in the case of three green algæ (*Chlorothecium saccharophilum*, *Chlorella protothecoides*, *Stichococcus bacillaris*), upon whose nutrition the effect of glucose was

* 'Bull. d. l. Soc. Imp. des Nat. de Moscou,' 1899, p. 39.

† Laurent, 'Ann. de l'Inst. Past.,' vol. 3, 1899; Lutz, 'Comptes Rendus,' vol. 126, p. 1227.

‡ Wanklyn, 'Chem. Soc. Journ.,' 1867, p. 59.

studied in pure culture, that there was a marked increase in growth when this carbohydrate was added to their culture medium, though no quantitative experiments were made to measure any difference in assimilation.

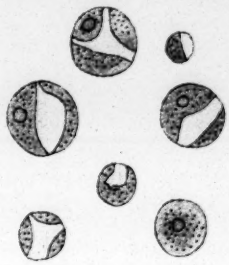
The general features of the assimilation of nitrogen, on the other hand, and its subsequent history displayed by *Chlorella pyrenoidosa*, may be a specialised characteristic of this plant, having reference to its continual occurrence, in nature, in waters which contain a comparatively large amount of ammonia, notably in sewage and sewage-polluted waters. It is, however, impossible to express a definite opinion upon this point until the nitrogen assimilation of a series of many other plants has been studied in a similar manner.

DESCRIPTION OF PLATE 8.

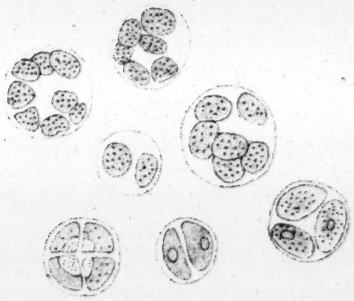
Fig. 1.—Culture on nutrient agar (1 month).
„ 2.—Culture on glucose nutrient agar (1 month).
„ 3.—Cells from a culture on nutrient agar of 10 weeks.
„ 4.—Cells from a culture on glucose agar of 3 weeks.
„ 5.—Cells dividing, from a culture on nutrient agar of 10 weeks.



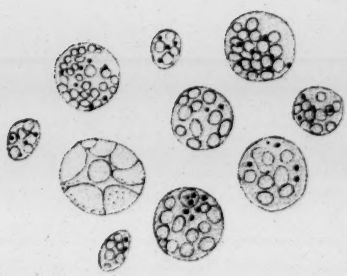
1. 2.



3.



5.



4.

Chick del.

Huth, Lith^r London.

"On *Lagenostoma Lomaxi*, the Seed of *Lyginodendron*." By F. W. Oliver, D.Sc., F.L.S., and D. H. Scott, M.A., Ph.D., F.R.S. Received March 19,—Read May 7, 1903.

The existence in Palæozoic times of a group of plants (the Cycadofilices of Potonié) combining certain characters of Ferns and Gymnosperms, has been recognised for some years past by various palæobotanists.* The group in question embraces a number of genera, among which *Medullosa*, *Heterangium*, *Calamopitys* and *Lyginodendron* may be mentioned; the fern-like foliage of these plants is placed according to its external characters in the form-genera *Alethopteris*, *Neuropteris*, *Sphenopteris*, and others.

The evidence for the intermediate position of the Cycadofilices is extremely strong, but at present it is drawn entirely from a detailed comparison of their vegetative organs, especially as regards their anatomical characters. In no case, as yet, is the fructification of any member of the group known with certainty; such indications as have hitherto been detected are still in need of corroboration. Thus, the suggestion has been made that the large seed, *Trigonocarpon olivæforme*, may have belonged to some member of the genus *Medullosa*;† and in the case of *Lyginodendron* itself there is fairly strong reason to believe that one form of fructification (in the light of the observations to be described below, presumably the male) may have been of the *Calymmatotheca* type,‡ a type, however, of which the organisation is not yet fully understood. In the absence of satisfactory data as to the fructification, so high an authority as M. Zeiller has expressed a doubt whether the Cycadofilices were, after all, anything more than a specialised group of Ferns.§

A re-examination of the seeds, placed by Williamson in his genus *Lagenostoma*, has revealed unexpected points of agreement between the structure of the envelopes of certain of these seeds, on the one hand, and that of the vegetative organs of *Lyginodendron* on the other.

Two species of *Lagenostoma* (*L. ovoides* and *L. physoides*) were

* Williamson, "Organisation of the Fossil Plants of the Coal-measures, Part XIII," 'Phil. Trans.,' B, vol. 178, p. 299; 1887; Solms-Laubach, 'Fossil Botany,' 1887, Engl. ed., pp. 141 and 163; Williamson and Scott, "Further Observations on the Organisation of the Fossil Plants of the Coal-measures, Part III," 'Phil. Trans.,' B, vol. 186, p. 769, 1895; Potonié, 'Lehrbuch der Pflanzenpalæontologie,' p. 160, 1899; Scott, 'Studies in Fossil Botany,' pp. 307 and 514, 1900.

† G. Wild, "On *Trigonocarpon olivæforme*," 'Manchester Geol. Soc. Trans.,' vol. 26, p. 434, 1900.

‡ Scott, 'Studies' p. 334; Miss Benson, "The Fructification of *Lyginodendron Oldhamium*," 'Ann. of Bot.,' vol. 16, p. 575, 1902.

§ 'Zeiller, 'Éléments de Paléobotanique,' 1900, p. 370.

described by Williamson;* a third species, the subject of the present note, was left undescribed by him, though in his MS. catalogue he named it, after its discoverer, *Lagenostoma Lomaxi*, a name which we here provisionally adopt. This seed occurs in calcareous nodules of the lower Coal-measures, and chiefly at Dulesgate, in Lancashire.

In general structure the seed *L. Lomaxi* agrees with *L. ovoides*.

It is an orthotropous seed, circular in transverse section, and broadest midway between base and apex. The height of the seed slightly exceeds the diameter, and in general form it may be compared with a Jaffa orange. Its height in full-sized specimens is about $5\frac{1}{2}$ mm., the diameter at the equator $4\frac{1}{4}$ mm. Many of the specimens that have passed through our hands show signs of having become detached through the agency of a layer of separation and bear a low conical papilla centrally placed at the chalazal end, beneath which the actual layer of abscission was situated.

In the most general relations of its organisation the seed approaches the Gymnosperm type in that the integument and nucellus are distinct from one another in the apical region only, whilst the body of the seed, which contains the large single macrospore with traces of prothallial tissue, shows complete fusion of the integumental and nucellar tissues. But in other respects the seed is remarkable. The free portion of the nucellus which stands above the macrospore is conical in form; its base is about 0·75 mm. across, and its height somewhat greater. The tapering apex reaches to the exterior, plugging the micropylar aperture like a cork. The whole of this structure, the "lagenostome" of Williamson, constitutes a pollen-chamber, owing to the separation of the nucellar epidermis from the underlying parenchymatous body of the free part of the nucellus. The pollen-chamber thus has the form of a bell-shaped cleft situated between the persistent epidermis and the central cone of nucellar tissue. Access to the chamber is gained at the apex, which is open, and pollen-grains are found in its lower part. The integument, which is a simple shell where fused with the nucellus, becomes massive and complicated in its free part, which corresponds to the upper fifth of the seed. In this region it is usually composed of nine chambers radially disposed around the micropyle. The existence of these chambers is indicated on the outside surface of the seed by the presence of nine little ridges disposed like the rays of a star around the micropyle, but dying out almost at once. These ridges over-lie the partitions of the chambered portion of the integument just as do the stigmatic bands the septa of a poppy capsule. The whole structure from within is like a fluted dome or canopy, the convexities of which correspond to the chambers,

* "Organisation," Part VIII, 'Phil. Trans.,' vol. 167, p. 233, figs. 53—75 and 77—79, 1877; Part X, 'Phil. Trans.,' Part II, 1880, p. 517, figs. 61—63. See Oliver, 'New Phytologist,' vol. 1, p. 145, 1902.

and actually engage with broad low grooves on the surface of the wall of the pollen-chamber.

The vascular system of the seed enters as a single supply-bundle at the chalazal papilla, and branches, a little below the base of the macrospore, into nine radially-running bundles. Each of these bundles passes, without further branching, to the apex of the seed, running outside the macrospore and a little distance below the surface. At the canopy the bundles enter the chambers and end at the tips.

Lagenostoma Lomaxi was thus a seed or seed-like structure, detached as a whole and containing pollen-grains in the remarkable cleft-like pollen-chamber; the integument in its free part, when compared with that of Williamson's *Lagenostoma physoides*, suggests a number of originally free arms or processes that have become laterally fused into a complex, chambered organ.

The seed, *L. Lomaxi*, is in some cases still attached to its pedicel;* the great peculiarity of this seed, as compared with other members of the genus, is that when young, and sometimes even at maturity, it is found enclosed in an envelope or cupule, springing from the pedicel just below the base of the seed, and extending above the micropyle—at least in young specimens. The cupule appears to have been ribbed below, and deeply lobed in its upper part; in form it may be roughly compared to the husk of a hazel-nut—of course on a very small scale.

The pedicel and cupule bear numerous capitate glands, of which some are practically sessile, others shortly stalked, while in others again the stalk is of considerable length. The head, or secreting portion of the gland, which is spherical in form, is almost invariably empty, only the multicellular wall persisting. The tissue of the stalk of the gland, consisting of many layers of cells, is preserved, though in a somewhat disorganised state.

These cupular glands present the closest agreement in size, form, and structure with the glands which occur on the vegetative organs of *Lyginodendron Oldhamium*,† and which are especially abundant on the particular form of that plant found in association with *Lagenostoma Lomaxi*. Both on petiole and cupule the majority of the glands are short, those which are not sessile being commonly about 0·4 mm. in height. Long-stalked glands, exceeding a millimetre in height, sometimes occur both on the vegetative organs and on the cupule. The dimensions of the head of the gland agree exactly on cupule and petiole, the diameter averaging about 0·2 mm. in each case. In both the stalk is usually somewhat narrower than the head, except at the base, where it is often considerably enlarged. On the stem, as might

* *Cf.* Williamson, *loc. cit.*, Part VIII, fig. 68 (*L. ovoides*).

† It has long been realised that the name *Lyginodendron Oldhamium* characterises a type rather than a species. It is probable that the very glandular form occurring at Dulesgate may deserve specific rank.

be expected, the glands are usually somewhat larger than on petiole or cupule.

As a rule, the structure of the glands on the vegetative organs is well preserved, the secretory tissue in the head being perfect. But occasionally the vegetative glands are found in the same state of preservation as those on the cupule, with the head hollow, owing to disappearance of the secretory mass. Where we thus have the two organs in a corresponding state of preservation, the agreement between the vegetative glands of *Lyginodendron* and those on the cupule of *Lagenostoma Lomaxi* is found to be exact.

There is no other known plant from the Coal-measures with glands at all similar to those described, nor is it likely that any unknown Gymnosperm should so exactly resemble *Lyginodendron* in these characters. On the ground, then, of the glandular structure we are led to the conclusion that the seed *Lagenostoma Lomaxi* can have belonged to no other plant than *Lyginodendron Oldhamium*, and more particularly to the glandular form of that type with which the seed is associated.

The state of preservation of the glands and of the cupule as a whole, indicates clearly that this organ, as we find it, was in an effete condition, having, no doubt, already discharged its functions while the seed which it protected was still quite young.

The vascular system of the cupule was well developed, and is very fairly preserved. A number of bundles branched off from the main strand of the pedicel, and traversed the cupule throughout its whole extent. The structure of the large bundle, seen in the pedicel, agrees with that of a petiolar strand in *Lyginodendron*. The minute characters of the tracheides are also in close agreement with those observed in the xylem of the foliar organs of the same plant.

Hence, characters presented by the internal anatomical structure strengthen the conclusion drawn from a comparison of the glands, and thus further support the attribution of *Lagenostoma Lomaxi* to *Lyginodendron*.

The evidence thus indicates that in a transitional type, such as *Lyginodendron Oldhamium*, with leaves wholly fern-like in structure and form, but with decided Cycadean as well as Filicinean characters in the anatomy of stem and root, the seed habit had already been fully attained, as fully, at any rate, as in any known Palæozoic Gymnosperm. *Lyginodendron* retains, so far at least as its vegetative structure is concerned, the intermediate position already assigned to it, but, whereas the fern-like characters have hitherto seemed to preponderate, the discovery of the seed inclines the balance strongly on the Gymnospermous side. It is not likely that *Lyginodendron* stood alone in this; we must now be prepared to find, what has long been recognised as a possibility, that many of the plants grouped under Cycadofilices already possessed seeds, and thus that a considerable proportion of the

so-called "fern-fronds" of the Palæobotanist really belonged to Spermophyta. It is at present impossible to say at what stage in the evolution of the Fern-Cycad phylum, the great change in reproductive methods came, whether it followed in the wake of general anatomical advance, or *vice versâ*. The discovery of further evidence as to the reproductive processes of these ancient plants may be expected to yield interesting results.

The authors are much indebted to Miss Marie Stopes for her valuable aid in the examination of the numerous sections in the Williamson and various other Collections.

Mr. James Lomax deserves high praise for his good judgment and skill in collecting and preparing the material for the investigation.

A full account of the fossils dealt with in the present note is in preparation, and will shortly be submitted to the Royal Society.

"On the Physiological Action of the Poison of the Hydrophidæ." By LEONARD ROGERS, M.D., B.S. (Lond.), M.R.C.P., F.R.C.S., lately officiating Professor of Pathology, Medical College, Calcutta. Communicated by Major A. ALCOCK, F.R.S. Received March 31,—Read May 7, 1903.

It has long been known that the great group of the Hydrophidæ, or Sea-snakes, are poisonous, and cases of death produced by their bites have been recorded, for example, that in Sir Joseph Fayrer's work on the Poisonous Snakes of India, of the ship's captain bitten while bathing in the Bay of Bengal, with a fatal result. The fishermen on this coast are also well aware of the danger of the bites of these reptiles, and take such good care to avoid them, that deaths among them are quite uncommon as far as I can ascertain. Deaths, however, not very rarely occur among those employed in oyster fisheries in shallow water in some places on the Madras coast, owing to snakes being trodden on, so that a study of the nature of the poison of this class of snakes has a practical as well as a scientific side, and, as far as I can gather from the literature of the subject obtainable in Calcutta, it has not yet received much attention. During the last year I have been investigating the subject, and although the amount of poison I have been able to obtain has been very small, yet it has sufficed to allow of certain definite results being obtained, which will be summarised in the following paper.

The Collection of the Poison.

The Hydrophidæ are met with in large numbers all round the coasts of the Indian peninsula, and have been specially studied at Puri on the east coast in Orrisa. It was at this place that I obtained

my specimens, which are caught by the fishermen in their nets during the calm cold-weather months with a frequency which is in proportion to the number of fish taken. By small payments they were induced to bring them to a tank which I had constructed near the beach, in which they usually only lived a few days, although some survived several weeks. By making them bite on a watch-glass covered with a thin layer of guttapercha tissue stretched tightly across it, they eject the poison into the glass as clear drops free from all saliva. This is then dried over calcium chloride or strong sulphuric acid, and can then be kept indefinitely in dry well-corked glass tubes, without losing its potency. The snake which is met with in greatest abundance in Puri is the *Enhydrina Bengalensis*, measuring from three to five feet in length, and it has a thick body and a large head. This species also furnishes the largest amount of poison, and from this alone have I yet been able to obtain a sufficient quantity to allow of a considerable number of experiments being performed with it. That of four other species, belonging to three different genera, has also been obtained in small quantities, so that four out of the six genera of Indian Hydrophidæ have now been examined, and will be dealt with.

Appearance and Quantity Ejected.

When the clear watery drop of poison is dried, it forms white shining scales, freely soluble in water or normal salt solutions, and differing from the poisons of Cobra and Daboia by the absence of the yellow tinge of the latter. The only exception I have met with was a faint yellow tinge in the dried poison of a *Disteira cyanocincta*, the others having all been colourless.

The quantity of poison ejected at a single bite is of great importance in relationship to the deadliness of these snakes, and fortunately it is very small. In many of the smaller species it is often impossible to get a drop at all, but probably when free in the water they can eject more poison than when being held close behind the head, with consequent great limitations of their power of motion. The amount of dried poison obtained from a single bite of thirteen different fresh specimens of the Enhydrina was weighed, and the average quantity was found to be 0·0094 gramme, or almost one centigramme. This is very much less than that obtainable from a Cobra or a Daboia, for the average amount of poison (dried) obtained from a Cobra is, according to D. D. Cunningham, 0·254 gramme, or twenty-five times as much as is obtained from an Enhydrina. In fact, so small is the amount, that at the end of a season I had only been able to obtain about one-third of a gramme of the latter poison, and for most of which I am greatly indebted to Dr. Reid of Puri. The poison also appears to be slowly formed, as a week after a snake had been made to bite, it is usually

impossible to get any further poison from it, even if it bite vigorously. Yet if made to bite a small fish immediately after ejecting his poison, the bite is fatal in a short time, showing how fatal a trace of it is. The largest amount of poison obtained at a single bite was 0·023 gramme. The other species mostly gave a smaller quantity than the Enhydrina.

Effect of Heat on the Poison.

On boiling a dilute solution of the poison, it becomes slightly opalescent. After being boiled for 15 seconds, two minimal lethal doses were recovered from, after slight symptoms had appeared, but four minimal lethal doses proved fatal in a somewhat longer time than with unheated poisons. After boiling for one minute, four minimal lethal doses were recovered from after only slight symptoms. Thus the poison is readily destroyed by boiling for a short time, but merely bringing it to the boiling point does not materially affect its strength. Some similar experiments with Cobra poison show that the latter is slightly more resistent to heat than is that of the Enhydrina.

Symptoms Produced by the Poison.

The following symptoms are common to all the species yet tested, no differences having been met with, except with regard to the exact amount of the minimal lethal doses in different animals, which will be dealt with presently. Briefly, the symptoms produced by the poison of the Hydrophidæ may be said to be identical with those caused by cobra venom, with one very important exception, namely, that the former venom has no appreciable action on the blood, which is a marked feature of cobra toxin. In the case of warm-blooded animals, such as rabbits, rats or birds, the symptoms produced by sea-snake poisons are as follows. When minimal lethal, or slightly supra-minimal lethal doses are given subcutaneously, there is always a long period before any symptoms of poisoning occur, the time varying in accordance with the dose from half an hour to several hours, in which respect it resembles Cobra, and differs markedly from Daboia venom. If large doses are given, the symptoms set in much earlier, and in that case death rapidly results. The symptoms are best studied by the use of small doses, when the first thing noticed is that the animal remains quietly in one position, and soon begins to show signs of drowsiness, closing its eyes at intervals. Next it begins to nod its head, but every now and then appears to wake up again and opens its eyes. In the case of birds—in which the symptoms can be best seen—the subject of the experiment next sits down on the floor of the cage, and although it can be made to stand up if disturbed, yet there is now evident commencing muscular weakness, and it can only walk with an

unsteady gait. By the time this stage is reached, it will be found that the animal is breathing more deeply than normal, while the number of respirations is also increased to a variable, but often considerable, degree. From this time the picture is one of progressive paralysis, affecting all the muscles of the body, and ending with respiratory convulsions. The animal nods more and more deeply, until the nose or beak touches the floor of the cage, only to be raised again with a jerky motion. It is now unable to stand upright, and the eyes remain closed. The respirations are now very deep and laboured, and in case of birds, the beak is half open, and gaping takes place with every inspiration, while the head is more and more lowered until its vertex instead of the beak rests on the floor, and the animal is unable to raise its head. Very soon after this stage of paralysis is reached, convulsions set in, and the respirations immediately fall very greatly in frequency, while they remain deep in character, although less regularly so than before, some being shallow, so that Cheyne-Stokes breathing is somewhat simulated. The convulsions recur, and soon respiration entirely ceases, but the heart continues beating for some time, usually two or three minutes in the case of warm-blooded animals, after the breathing has entirely ceased. When the convulsions commence, the animal rolls over on its side in a state of nearly complete paralysis. Every word of the above description of the symptoms produced by the poison of the Hydrophidæ is equally true of Cobra poisoning, so much so that if two animals are severally given minimal lethal doses of these two poisons, it is impossible to distinguish which animal has received which poison by the clinical symptoms produced, a fact which I have repeatedly demonstrated.

Post mortem, after death from the poison of the Hydrophidæ, there is little or nothing noteworthy found. The seat of injection is free from extravasation of blood, and presents little or no serous effusion. The blood is of a dark colour, no doubt due to the respiratory paralysis. It is fluid on opening the heart, but rapidly clots when placed in a small test-tube, doubtless owing to the large amount of CO_2 gas in it. On standing it exudes serum, which is usually clear, but may be very slightly blood-stained, although very much less so than in the case of Cobra-poisoning under the same circumstances. There is no intravascular clotting to be found *post mortem* in the portal or other veins, as C. J. Martin first demonstrated in Pseudechis poisoning, and as occurs in acute Daboia poisoning as recently shown by Lamb. No other naked-eye changes have been found after death from sea-snake poisons.

In the case of cold-blooded animals, such as fish, which have frequently been used in these experiments, the symptoms are essentially the same in kind as in warm-blooded animals, although less easy to observe. After small doses there is the same long latent period, often

lasting for several hours. Sometimes temporary excitement with rapid motion may be observed for a short time, but more often the picture is simply one of slowly progressing paralysis. In most kinds of fish this is also very well shown by a gradually increasing difficulty in maintaining the upright position, the fish slowly turning over on one side and then swimming up into its upright position again, only to slowly sink on to its side once more. The respirations will now be found to be deeper than normal, although not as a rule quicker, but, on the contrary, they steadily slow down from the beginning of the symptoms to the end without any marked increase in the rate. This paralysis of all the muscles and of the respirations steadily progresses until convulsions set in, to be immediately followed by a very rapid failure of the respirations both in number and depth, so that they become difficult to detect, and death soon follows. The heart will be found beating some time after the breathing has ceased, and no extravasation of blood or other noteworthy change is found *post mortem*. Here again the symptoms are precisely similar in poisoning of fish by Cobra venom.

The Potency of the Poison.

By working out the smallest fatal doses of the poison per kilogramme of weight in different animals, and comparing them with those obtained by former workers for other snake venoms, we shall be able to estimate the potency of that now being dealt with. This has been done in the case of the poison of the Enhydrina by means of numerous experiments carried out with the mixed dried venom of a number of these snakes, with the following results. At the same time comparative experiments were also carried out with fresh dried Cobra venom for comparative purposes. White rats were first tested, and 0·07 milligramme per kilo weight was found to prove fatal, but smaller doses were sometimes recovered from. In the case of Cobra poison 0·5 milligramme per kilo was necessary to produce death, while Lamb in Bombay found the fatal dose of this poison for rats to be 0·33 milligramme. It is evident then that the poison of the Enhydrina is several times as potent as is Cobra venom on rats. In the case of rabbits only a few experiments have been performed, but 0·04 milligramme per kilo proved fatal in under four hours in one case, while in another 0·01 milligramme per kilo produced no symptoms but loss of appetite; but on giving a second dose of 0·02 milligramme per kilo five days later (the animal having fully recovered from the first dose in one day), death resulted in a few hours. On the other hand, Elliot found the minimal lethal dose of Cobra venom for rabbits to be 0·7 milligramme per kilo weight, so that it is evident that these animals are many times as suscep-

tible to the poison of Enhydrina as to that of Cobra, the former poison being some twenty times as potent for them as the latter—a remarkable difference.

A larger number of experiments have been carried out with birds, pigeons and fowls. These also bear out the former ones in proving the far greater potency of the poison of the Enhydrina over that of the Cobra or other poisonous snake yet examined. In the case of pigeons the minimal lethal dose, 0·05 milligramme per kilogramme, always proves fatal, while in fowls the fatal dose is 0·04. These figures may be compared with those obtained by D. D. Cunningham in his numerous experiments with Cobra venom on fowls, for which he found the minimal lethal dose to be 0·5 milligramme per kilo, so that the poison of the Enhydrina for birds is at least ten times as potent as is Cobra venom, which goes far towards neutralising the effect of the much smaller dose of poison ejected by the Enhydrina as compared with the Cobra. Taking the minimal lethal dose of the Enhydrina for warm-blooded animals as 0·05 milligramme per kilo, the fatal dose for an average man of 70 kilogrammes would be 3·5 milligrammes, or about one-third of the average amount of venom ejected by a fresh full-grown specimen of this, by far the most commonly met with, kind of snake in the Bay of Bengal. There is good ground, then, for the belief in the deadliness of the Hydrophidæ.

The Minimal Lethal Dose for Fish.

It is well known that it is necessary to give many times as large a dose of Cobra venom, in proportion to the weight of the animal, in order to kill cold-blooded animals as is required for destroying the life of warm-blooded animals. Now there is no doubt that the Enhydrina live on fish, and I have been able to ascertain that they can swallow those of considerable size. One specimen of Enhydrina after being handled in the process of taking poison vomited a piece of half-digested fish, which on comparison with complete fish of the same kind was found to have certainly been a foot or more in length, while it was over 2 inches in depth. Such a fish could not have been swallowed if it had not first been killed, or at least paralysed to a marked degree. It is of interest, then, to ascertain the minimal lethal dose of these snakes against fish, and to compare it with that of the Cobra. As I have not been able to find accurate records of the effect of Cobra venom on fish, I have also ascertained this by a series of experiments, using the hardy Mud-fish (*Saccobranchus fossilis*), which lives for weeks in a small vessel of water. It was found that 25 milligrammes per kilo of Cobra venom had to be given to be certain of causing death, although sometimes a slightly smaller dose was effective. Thus fifty times as much Cobra venom is required to

kill a fish as is sufficient to kill a warm-blooded animal—a very marked difference. On testing the same species of fish with the poison of the Enhydrina, it was found that 0·5 milligramme per kilo of freshly dissolved poison was always fatal, and sometimes a smaller dose caused death. Thus the dose of this sea-snake poison required to kill fish was but ten times as much as the minimal fatal dose for warm-blooded animals, that is, considerably less than we found to be the case with Cobra poison. In other words, the poison of the Enhydrina is much more deadly than is Cobra venom for fish, even allowing for the greater potency of the former for warm-blooded animals, so that it appears to be specially adapted for the needs of the Sea-snake, which lives on fish, being in all about fifty times as potent for fish as is Cobra venom. This great concentration of the poison may be of considerable advantage to the reptile when dealing with such active prey as fish in their own element. This special affinity of the poison for fish was even more marked in the case of some of the other species tested. Thus, that of a single species of the *Disteira cyanocincta* was fatal to pigeons in doses of 0·5 milligramme per kilo, being thus considerably weaker than that of the Enhydrina, but only 1 milligramme per kilo was required to kill fish, that is but twice as much as was needed to kill birds. Similarly with the *Disteira viperina* the minimal lethal dose for pigeons was 0·5 milligramme, and for fish only 0·75, or but very little more. Again, the poison of the *Hydrophis cantoris* for both pigeons and fish was just the same as the last-mentioned species. Lastly, the poison of the *Hydrus platurus* killed pigeons in doses of 0·075 and fish in one of 0·25 milligramme per kilo, being thus very deadly for both cold and warm blooded animals. The above include four out of the six genera of Hydrophidæ found in Indian waters, so that, although the poison obtained from the last four species was from single specimens, and therefore cannot be taken as more than approximately accurate, yet they suffice to prove that the Hydrophidæ as a class secrete very virulent poisons, which are specially poisonous to fish. It is also worthy of note that the two genera which proved to be most deadly to warm-blooded animals, namely, the Enhydrina and the *Hydrus platurus*, are just the two which the fishermen at Puri said were the most dangerous ones, as the accuracy of their statement points to actual experience in the human subject of their deadliness having been handed down among them. Some of the smaller species, however, probably do not eject sufficient poison to prove fatal, to adults at any rate, and hence are not so much dreaded by the fishermen. It will also be observed that the poison of the *Enhydrina Bengalensis* is the most potent of those so far tested, while it also yields the greatest amount of poison, with the exception, perhaps, of the *Disteira cyanocincta*.

Effect of the Poison on other Cold-blooded Animals.

I have not yet been able to test any extensive series of other cold-blooded animals to see if they are equally susceptible to the poison of the Hydrophidæ as fish are, but in one instance a frog weighing 30 grammes was injected with a dose of 0·2 milligramme per kilo, with the result that it showed well-marked symptoms of paralysis, but eventually recovered, so that it would appear to have been about as susceptible as fish. Some harmless snakes were injected with noteworthy results. Thus, two specimens of the *Coluber fasciolatus* were injected with doses of 10 and 50 grammes per kilo respectively, with the poison of the Enhydrina, with no ill effect, and the former received a second dose of 50 milligrammes per kilo three days after the smaller dose, equally without effect. Here we have a harmless colubrine snake withstanding 100 times the fatal dose for a fish and 1000 times that for a warm-blooded animal. Further, two specimens of the harmless green Whip-snake (*Dryophis mycterizans*) were tested, but in this species 25 milligrammes per kilo in one instance, and 15 in the other, each produced death in less than two hours, so that a smaller dose would nearly certainly have been fatal. This opens up a large question which must await further investigation.

The Physiological Action of the Poison on the Blood.

The striking similarity of the symptoms produced by the poison of the Hydrophidæ and by Cobra venom leads one to expect a similarity of action on the blood. The researches of Cunningham have shown that Cobra poison has a very marked power of dissolving the red corpuscles of the blood and also in reducing its coagulability, and, contrary to the views of Lauder Brunton and Fayrer, he holds that these blood changes are the essential features of the action of the poison, and not its action on the nervous system, as held by the latter authors. Experiments have been carried out to test the effect of the poison of the Hydrophidæ on the blood, with unexpected and important results. Taking first the poison of the Enhydrina, with which most of the observations have been made, and remembering that it is ten times as potent for warm-blooded animals as is Cobra venom, we may compare the action of the two poisons in dissolving the red corpuscles of the warm-blooded animals, the blood of pigeons and of the human species having been used in the experiments. The method of mixing the poison in different degrees of dilution with a minute measured drop of blood, and counting the number of corpuscles with a hæmocytometer before, and at varying periods after, the addition of the venom was adopted. The poisons were always dissolved in isotonic salt solutions, and equal quantities of blood in the same salt solution,

but without the addition of the venom, used as controls. These control solutions showed no dissolution of the red corpuscles after twenty-four hours. From 5 to 10 cubic centigrammes of blood were added to from $\frac{1}{2}$ to 1 c.c. of the isotonic solution of the poison, varying strengths of the latter being tested in this way. Pigeon's blood is specially well suited for these experiments, as the bodies of the corpuscles are dissolved while the nuclei remain visible. It was found that a 1-in-1000 solution of Cobra venom (1 milligramme in 1 c.c.) produced a very rapid solution of the red corpuscles, which had all disappeared in seven minutes. A 1-in-20,000 solution took a much longer time to produce complete dissolution, namely two and a half hours. In the case of human blood a 1-in-10,000 solution of cobra venom dissolved the whole of the red corpuscles in from fifteen to thirty minutes, while one of a strength of 1 in 20,000 took about one hour to do so. A 1-in-100,000 solution had very much less effect, having produced only a slight diminution in the number of the red corpuscles within one hour's time. The white corpuscles were not dissolved by the venom in the strengths used.

Let us now compare these data with those obtained with the poison of the Enhydrina, bearing in mind the much greater potency of the latter as compared with Cobra venom. The poison of the Enhydrina was mixed in the same way as above described with the blood of pigeons and with human blood, in strengths of 1 in 1000, with the result that at the end of one or two hours there had been no appreciable dissolution of the red corpuscles. On testing again several hours later, slight dissolution was found to have taken place, and by this time the solution also showed naked-eye evidence of commencing hæmolysis. After having been kept at room temperature (from 70° to 80° F.) for twenty-four hours the dissolution appeared to be complete, but, on examination with the microscope, a few red corpuscles were still found to be undissolved, showing that even after this lapse of time the hæmolytic change was not quite complete. The poison of the *Disteira cyanocincta* and the *Hydrophis cantoris* were also tested in the same way with precisely similar results, namely, that a strength of 1 in 1000 had no appreciable hæmolytic effect at the end of one hour, but caused nearly complete dissolution at the end of the course of twenty-four hours. This is about the same effect as is brought about by a solution of Cobra venom of a strength of 1 in 100,000, although Cobra venom has a potency of only one-tenth that of the poison of the Enhydrina. Thus we find that in proportion to its potency the poison of the Cobra has about 1000 times as great a hæmolytic effect on the red corpuscles of warm-blooded animals as has that of the Enhydrina. We have already seen that the latter poison produces no blood-stained effusion at the site of the injection of a fatal dose, evidently on account of the strengths used

having no hæmolytic action, for the solutions employed for the small animals experimented on were 1 in 10,000 or less. If we work out the amount of poison required to dissolve a certain amount of the blood of a pigeon, for example, we find that it takes about 200 times a fatal dose to dissolve 1/2000th part of the bird's blood in twenty-four hours, calculating this fluid to be one-thirteenth of its body weight. It is obvious, then, that ordinary fatal doses of the poison of the Hydrophidæ can have no appreciable hæmolytic effect, and that death cannot be attributed, even in a partial degree, to its action on the blood of the animal killed by it. This can also be demonstrated by another method of experiment, namely, by counting the number of the red corpuscles before the administration of the fatal dose of the poison, and again immediately after death. This I have done several times, with the result of showing that no dissolution of the red corpuscles resulted from the action of a lethal dose of the Enhydrina poison. For example, a fowl's blood was counted, and 3,190,000 red corpuscles per cubic millimetre were found. A lethal dose of Enhydrina poison was then injected subcutaneously, which proved fatal in just one hour, when the blood count showed 3,120,000 red corpuscles in the same quantity of blood.

Next we have to deal with the action of the poison on the coagulability of the blood. In the case of Cobra venom marked changes are produced, as shown by D. D. Cunningham, and this point has recently been studied by Lamb. The virus has the action of reducing or totally destroying the clotting power of the blood when mixed with it in small quantities. I have made a few observations on this point with the following results. Wright's tubes were used, the solution of the poison being first drawn up into them, and then an equal quantity of the blood drawn up and quickly mixed with the venom solution in the mixing chamber, and blown down into the tube again, and the conditions as regards clotting examined in a series of such tubes at given intervals. The clotting time, when mixed with an equal quantity of the normal salt solution (in which the venom was also dissolved) of a rabbit, having first been found to be three minutes, those of different strengths of Cobra venom in normal salt solution were found to be as follows: when a 1-in-10,000 solution was added the coagulation time was seven and a half minutes; with 1-in-1000 solution it was twenty minutes, and with a 1-in-200 one the blood was still quite fluid after twenty-four hours, its coagulability having been completely destroyed. On testing the effect of the poison of the Enhydrina in a similar manner it was found that a 1-in-1000 solution had no effect in reducing the coagulability of the blood, which still clotted solid in three minutes; when a 1-in-200 solution was added the blood still clotted in five minutes, showing only a slightly reduced time with the same strength, which in the case of Cobra venom had completely

destroyed the clotting power, and this, too, it must be remembered, in spite of the Enhydrina poison being ten times as powerful as that of the Cobra. It is evident, therefore, that the poison of the Enhydrina has no appreciable effect in ordinary dilute minimal lethal doses on the coagulability of the blood, while, as a matter of fact, we have already seen that such doses do not produce any loss of the clotting power of the blood. This was also the case when fifty times a minimal lethal dose of the venom was injected into the vessels of rabbits with the result of causing death in about six minutes.

The above experiments show that the poison of the Hydrophidæ has no appreciable action on the blood of animals, which can in any way account for the symptoms and fatality caused by it, yet it kills with precisely the same symptoms as are produced by Cobra venom, and, as we shall see presently, there are good reasons for believing that it has a special action on the nervous system. It will be evident at once that this furnishes a very strong argument in favour of the view that Cobra venom also kills through the nervous system, as held by Lauder Brunton and Fayrer, and not through the blood, as maintained by Cunningham. It is also of special interest to observe that although the action of the poison of the Hydrophidæ on the blood is practically a negligible quantity in its lethal effects, yet it still persists to a slight, but easily demonstrable, degree; for if it so persists in the Sea-snake, it may also persist in a still greater degree in the case of the Cobra without being a very active agent in the lethal effects produced by that poison, which kills through the nervous system as does that of the Hydrophidæ. In this connection it is interesting to observe that all through the poisonous snakes we find evidence of an action on the blood and on the nervous system in different degrees. Thus, beginning with the Viperine snakes, we first have the *Vipera Russellii*, which appears to be the purest blood poison of the known venomous snakes, killing by producing intravascular clotting in large doses, and the opposite effect of total loss of coagulability in repeated sub-minimal lethal ones. Then we come to the class of Pit-vipers, of which the rattlesnake of America has been most closely investigated by Weir Mitchell and Reichert. They also found a very marked effect on the blood, apparently similar to that produced by the Daboia, but, combined with this, we have a marked paralytic effect on the nervous system, and especially on the respiratory centre, for the authors mentioned conclude that although death may occur through the effect on the blood, yet they add "There can be no question, however, that the respiratory centres are the parts of the nervous systems most vulnerable to the poison, and that death is commonly due to their paralysis." Leaving the Viperine snakes and passing on to the poisonous Colubrines, we first come to the Australian species, so ably studied by C. J. Martin, namely, the Pseudechis, and we find again a combination of the two effects to

such a marked degree that, when the venom is administered intravenously, death results from intravascular clotting, as in the Viperine snakes, while if minimal lethal doses are given subcutaneously death results through paralysis of the respiratory centres. Next we come to the Cobra, another Colubrine snake, and here we find the nerve symptoms quite predominate, although some considerable effect on the blood in the form of reduction of coagulability and dissolution of the red corpuscles still survives, although it now takes quite a secondary position to the effect on the nervous system. Lastly, we have the Hydrophidæ, which, morphologically considered, are but colubrines modified for an aquatic existence, and here we find a practically pure nervous poison, although there still persists a trace of action on the blood if strong solutions of the venom are employed, although it can have no actively poisonous effect. The very slight action found, however, may be of some value to the snake in the following way. We have seen that a 1-in-200 solution of the Enhydrina poison has a slight retarding effect on the clotting power of the blood, which would doubtless be more marked in still more concentrated solutions, so that it is highly probable that the pure poison would have the effect of preventing the clotting of the blood at the point of injection of the poison, and so allow of its more ready absorption into the circulatory system through the patent vessels severed by the fang. This will account for the extreme rapidity of the absorption of the poison of the Cobra, for Fayrer showed long ago that if immediately after a dog has been bitten by this snake the fold of skin punctured is raised and freely excised, still the animal dies of the poison. The survival of some degree of action on the blood in the case of the Cobra and the Hydrophidæ, although not in itself an important element in directly causing the death of the animal, may nevertheless be of service in causing the venom to be more rapidly absorbed in the way just pointed out.

Action of the Poison on the Pulse and Respiration.

We have already seen that in slow poisoning the respirations become more and more laboured until convulsions set in and they quickly cease, while the heart continues to beat for a short time. For the accurate study of the exact effects on the respiratory and circulatory systems, proper recording apparatus is necessary, but as these were not available, I had to content myself with a record of the rate of the pulse and respiration after the intravascular injection of a large and rapidly fatal dose of the poison into rabbits under the influence of chloroform, with the following results. A dose of 1 milligramme per kilo. weight, or at least twenty times a minimal lethal dose was used, and death resulted in from six to eight minutes, taking the time up to

the cessation of the heart's beat. The effect on the respiration was simply a uniformly steady slowing down until convulsions set in, when the breathing finally ceased at once. For example, in a rabbit which had received a dose of 1 milligramme per kilo. directly into the carotid artery (the artery being clamped immediately afterwards to prevent hæmorrhage), the respirations were 60 per minute immediately before the injection of the poison. During the four minutes immediately following the injection, the number of respirations were as follows:—first minute, 56; second minute, 51; third minute, 42, and the fourth minute 33. In the first quarter of the 5th minute they were 8, at which point convulsions set in and the breathing stopped. The respirations were written down every quarter of a minute, and the figures for the separate quarters show an equally steady diminution of the number of respirations as the minute periods just given. In the same experiment the pulse showed the following changes. Before the injection it was 105 per minute. During the second half of the first minute after the injection it was 47 (that of the first half minute was lost), during the second minute it was 106, showing no alteration up to this time. During the third minute it fell to 99, and during the fourth it further fell to 48, that for its first half having been 32, and for the second half 16. During this steady fall in the pulse rate, its volume and force became increased. During the fourth minute, as already mentioned, convulsions set in, and the pulse was lost for about a minute, only the first and third quarters of the fifth minute having been recorded as 8 and 11 beats respectively. During the last three quarters of the sixth minute the beats were 15, 15 and 17 respectively, being now very feeble instead of unusually full, as before the cessation of respiration and onset of convulsions. During the first and second quarters of the seventh minute, the beats were 26 and 20 respectively, at which point the heart finally ceased to beat, that is, three and a half minutes after the cessation of the breathing. Very similar results have been obtained in another experiment, in which the same dose was injected into the jugular vein, a steady fall in the respirations first occurring, and they ceased with the onset of convulsions, while an equally steady fall in the pulse rate occurred later than that of the respirations, accompanied with an increased volume of the artery, the tension rapidly falling when the respiratory convulsions set in, but the pulse at the same time became more rapid again until it finally declined once more and then ceased. These experiments appear to show that the primary effect of the poison is a paralysing action on the respiratory centre, and that the cardiac failure is secondary to that of the respirations. The exact explanation of the slowing of the pulse with increased volume of the artery, I am not prepared to say without the aid of pressure tracings, which I have not yet been able to take.

The Affinity of the Nervous System for the Poison.

We have seen that the poison of the Enhydrina is much more potent than even that of Cobra, and it appears to be somewhere intermediate in virulence between Cobra and tetanus toxins. Further, we know that the repeated injection of gradually increasing doses of the latter two poisons into susceptible animals leads to the formation of an anti-toxin in the system. This marked similarity of the nerve poisons of the Colubrine Snakes and tetanus toxin leads one to inquire whether these snake venoms do not exert their noxious influences in the same way that tetanus toxin does, namely, by being taken up from the circulation and fixed in the nerve cells until a sufficient dose has been absorbed to paralyse the nervous matter. We know from the experiments of Wassermann that small amounts of tetanus toxin can be thus fixed by fresh nerve matter in a test-tube, and so rendered inert when subsequently injected into a susceptible animal. It seemed to be worth while to repeat these experiments with the poison of the Enhydrina, and although I have not had time to carry out a sufficiently exhaustive series of experiments to settle this point, yet the following data appear to me to have some value as being highly suggestive of the mode of action of these nerve-paralysing snake venoms.

The experiments were carried out in the following manner. A weak solution of the venom, such as is used when giving minimal lethal doses, was placed in a small sterilised test-tube, and a given quantity of fresh brain matter from a pigeon was added to it, and the whole kept at blood temperature for a given time. Another solution of the same strength was kept at the same temperature for an equal period of time without the addition of any brain matter, for the purpose of injecting control animals, which were always used. Double and quadruple minimal lethal doses were used, and the brain matter was broken up so as to mix it with the poison as intimately as possible, and subsequently injected without filtering, so that most of the brain matter in a fine emulsion was injected with the poison. It was found that pigeons injected with these emulsions always lived longer than the control one, while they sometimes recovered from double, and in one instance from quadruple, minimal lethal doses of the poison after being mixed for from half an hour to eighteen hours with a small quantity (from 3 to 20 centigrammes) of fresh brain matter. The most marked effects were obtained by the use of the hemispheres of the cerebrum, the instances of complete recovery from lethal doses having occurred in these instances. The cerebellum had a less marked effect, only considerable prolongation of life having occurred, while in one experiment with the medulla and pons no very marked effect was observed. The grey matter, then, appears to have most effect in fixing the poison, as is also the case with tetanus toxin. These experi-

ments, then, point to the action of the toxins of the Enhydrina being very similar in nature to that produced by the tetanus bacillus. A few experiments were also done with Cobra poison in the same way, using the cerebrum only, but here the results were not so marked as in the case of the Enhydrina poison, only a retardation of the onset of symptoms and of death having been observed.

Antitoxins.

Lastly, we have to deal with the question of the possibility of obtaining an antitoxin against the poison of the Hydrophidæ. It has now been abundantly proved that Calmette's antivenin is not a specific against all kinds of snake venom, as he claimed, although in large doses (40 c.c. according to Lamb) it is undoubtedly of great value against the poison of the Cobra. The very marked similarity of the symptoms of poisoning by the Hydrophidæ with that produced by Cobra, lead one to hope that the antitoxin, which is efficient against the latter, would also be of value against Sea-snake venom. This has been put to the test by adding minimal and slightly supraminimal lethal doses of the poison of the Enhydrina to one half c.c. of fresh Calmette's antivenin (which had only reached Calcutta a very short time before it was used), and after allowing the mixture to stand at blood heat for half an hour, injecting the whole subcutaneously. White rats were used in the experiments, and the amount of antivenin in proportion to the amount of poison was relatively enormous as compared with the dose recommended in the treatment of men bitten by venomous snakes. Yet the animals uniformly died in just about the same time as the controls, so that it is evident that Calmette's serum is of no use against the poison of the Hydrophidæ.

On the other hand, the similarity in the action of this poison to the Cobra and tetanus toxins leads one to expect that an antidote could be prepared against it in a similar way to those of the latter poisons. It is only during three months that I have been able to experiment on this point, fowls being used. It soon appeared that the doses had to be very slowly increased, or fatalities occurred, and in the limited time these experiments lasted, I was only able to immunise one fowl against the minimal lethal dose of this poison, and a slightly larger dose proved fatal with the usual symptoms. My intention was to immunise a series of animals against the Enhydrina poison, and then to test them with small doses of poisons from the other Hydrophidæ, as owing to the large variety of this class of snakes, no antidote would be of any practical value unless it was equally potent against all the genera and species, or at least against the ones most commonly met with. This important and interesting question must await further investigation.

One experiment, which was carried out in order to test if the serum

or bile of the Enhydrina had any antidotal properties, deserves mention in this connection. Three puppies of the same litter were used, all very much of the same size. Each received an equal quantity of Enhydrina poison, but in the first this was mixed with a four minims of the serum of the Enhydrina; in the second it was mixed with four minims of the bile of the same snake; and the third received the poison solution only as a control. The mixtures were injected ten minutes after being made. The result was that all three animals died in a little over an hour, the control surviving slightly longer than the others. It appears, then, that neither the serum nor the bile of this snake has any antidotal properties against the poison, and can not, therefore, be utilised in the treatment of their bites. Further research will be necessary to determine if a practically efficient antidote can be prepared, which I hope to undertake when sufficient venom for the purpose can be obtained.

This concludes the most important experiments so far carried out by me with the poison of the Hydrophidæ. They have necessarily been strictly limited by the very small amount of poison which I have yet been able to obtain, and by the equipment of the laboratory at my disposal, for the use of which I am indebted to the kind permission of the Committee of the Zoological Gardens of Calcutta. I am also indebted to the Bengal Government for a grant towards the expense of this investigation.

"Experiments in Hybridisation, with Special Reference to the Effect of Conditions on Dominance." By L. DONCASTER, B.A., King's College, Cambridge. Communicated by Dr. S. F HARMER, F.R.S. Received March 19,—Read May 7, 1903.

(Abstract.)

The paper describes experiments made at Naples with hybrid Echinoid larvæ. The object of the experiments was to determine whether the dominance of a character is influenced by the condition of the genital cells at the time of fertilisation. It had been suggested by Vernon* that the "prepotency" of the sexual cells varies with their maturity, and experiments were made to test this conclusion, and also to discover whether "prepotency" could be influenced by other conditions acting on the eggs or spermatozoa before fertilisation.

It was found that adverse conditions acting on the eggs did give rise to differences in the larvæ, but evidence is given to show that these differences are not due to a change in the dominance of characters, but are the result of differences in the vigour of the larvæ. It is also shown that the seasonal changes observed by Vernon are probably due chiefly, if not entirely, to differences of temperature, and are not caused by a change of dominance accompanying difference of maturity.

It is also shown that if an individual, A, shows greater dominance than B when each is crossed with a specimen X of the other sex, then A will also show greater dominance than B when both are crossed with a specimen Y.

It is shown that the different characters of one parent are inherited separately by the hybrid offspring, so that there is no pronounced correlation in the offspring between characters derived from the same parent. Further, a given character may appear in very different degrees on the two sides of the body of a hybrid larva, so that the hybrids are very frequently asymmetrical, although in the characters considered the two sides of the pure-bred larvæ are similar.

Experiments are described dealing with the causes which hinder cross-fertilisation between separate species, and it is shown that treatment of the eggs which tends to reduce their vitality usually renders their fertilisation by sperm of another species more easy.

* 'Phil. Trans.,' B, 1898.

"Preliminary Note on the Discovery of a Pigmy Elephant in the Pleistocene of Cyprus." By DOROTHY M. A. BATE. Communicated by HENRY WOODWARD, LL.D., F.R.S., F.G.S., V.P.Z.S., late Keeper of Geology, British Museum, Natural History. Received April 23,—Read May 7, 1903.

While still in Cyprus the receipt of a grant from the Royal Society in April, 1902, enabled me to devote a considerable amount of time not only to making more extensive excavations in some of the caves previously found, but also to a search for further cave deposits. I confined my attention chiefly to the Keryina range of limestone hills in the north of the island in the hope of finding bone caves containing other remains than those of the pigmy *Hippopotamus*, of which Dr. Forsyth Major has already given a short description* from specimens discovered by myself.

In this search I was at length successful, although it was not until a certain amount of tentative digging had been carried on in four out of five newly discovered deposits that work was started on what appeared at first to be the most unpromising looking place which had been found, and was consequently the last to receive attention.

However, during the first day one of the workmen found, not far from the surface, part of a tooth which was at once recognised as being that of an elephant. After this discovery every effort was made to procure a complete collection of the remains of this species, but at no time were either teeth or bones found to be so plentiful as those of *Hippopotamus minutus*, with which they were associated.

Often not a single proboscidean tooth would be obtained during two or three days' work, and only eleven molars and parts of molars were procured as the result of three weeks' digging. It was then decided to continue excavations here for a short while longer, and this was done until the end of July, work being again resumed in the beginning of the following October.

Altogether a good series was obtained of the teeth of this elephant, which is found to be a pigmy species. With the exception of the first milk molar (m.m. 2), specimens were procured of all the milk and permanent molars of both the upper and lower jaws; also a number of tusks of different sizes, though these included none of the tiny milk incisors. No teeth which could be referred to very aged individuals were obtained, for amongst the last true molars none have more than half their full number of plates in use.

The series of teeth consisting of specimens of very small size, it was natural in the first instance to compare them with the remains of the

* 'Proc. Zool. Soc.,' June 3rd, 1902.

dwarf species from the Pleistocene deposits of the caves and fissures of Malta and Sicily. It was thought probable that they would differ from these; the fact of the pigmy hippopotamus of Cyprus being distinct from those found in the other large Mediterranean islands, lending colour to the supposition; this expectation was fulfilled, for the Cyprus fossils do not appear to be identical with any of the Maltese species, though they seem to come nearest to *Elephas melitensis* both in size and in the number of plates in the molars. The number of these plates in any particular tooth is liable to vary to a certain extent, but on taking the average, as far as this can be judged from the amount of material available, the resulting ridge formula, exclusive of talons, is $\div \frac{5}{5}\ \frac{7\text{–}8}{7\text{–}8}\ \frac{8}{8}\ \frac{9}{9}\ \frac{12}{12}$ which practically agrees with that of *E. melitensis* given by Dr. Falconer.*

The teeth of the Cypriote elephant are considerably smaller than those of *E. mnaidriensis*, from both Sicily and Malta, this being the largest species from the last-named island. They also differ somewhat in their ridge formula, which is that mentioned above, while Dr. Leith Adams† gives that of *E. mnaidriensis* as $\frac{3}{3}\ \frac{6}{6}\ \frac{8\text{–}9}{8\text{–}9}\ \frac{8\text{–}9}{8\text{–}9}\ \frac{10}{10}\ \frac{12\text{–}13}{12\text{–}13}$.

The Cyprus form seems to have been also slightly inferior in size to *E. melitensis*, for its largest upper and lower molars do not equal, either in length or breadth, some of the specimens of the corresponding teeth of this Maltese species which are in the collection of the British Museum. Its tusks differ from all those from Malta in being compressed laterally, which character is especially noticeable in those of the female and young; further, they appear to be more strongly curved than those of *E. melitensis*.

As a general feature it may be said that the molars from Cyprus are, on the whole, more simply constructed than those of *E. melitensis*. They show a still slighter tendency to "crimping" in the bands of enamel, and are less inclined to develop the mesial expansion of the plates of dentine which is not uncommonly found in the teeth of *E. melitensis*, and is so conspicuous in those of *E. Africanus*.

It is well known that when the plates of an elephant's tooth first come into use, the edging of enamel is in the form of a series of rings owing to the digitation of the plates. These are later worn into a single band surrounding the enclosed area of dentine.

In the Maltese specimens it is not uncommon to find the encircling enamel persisting thus divided for a considerable time. Even four or five ridges may remain in this condition at one time in a single

* 'Pal. Mem.,' vol. 2, p. 298. London, 1868.

† 'Zool. Soc. Trans.,' vol. 9, 1874, p. 112.

tooth, with perhaps an anteriorly decreasing number of rings. This is well shown in a tooth, now in the British Museum collection, doubtfully ascribed by Mr. Busk* to the first upper true molar of *E. Falconeri*. This is not so much the case in the Cyprus specimens, in which the bands of enamel only remain thus separated into several annuli for a very short while after the plate comes into wear.

The molars vary considerably, some specimens having very broad crowns while others are somewhat narrow. The bands of cement are wide, in perhaps the majority of cases almost, or quite, equalling in width the plates of dentine; this seems to be the exception and not the rule in the molars of *E. melitensis*.

Taking into consideration the several characters in which the teeth of the Cyprus elephant differ from those of all the hitherto described dwarf species (putting on one side *E. lamarmoræ*† from the Pleistocene of Sardinia, the teeth of which are unknown to science) as well as the distinct habitat of the animal, I have come to the conclusion that it is specifically distinct from these other small forms, though possibly they were derived from a common ancestor, and I, therefore, propose to name it *Elephas cypriotes*.

The discovery of the remains of this pigmy elephant, as well as of *Hippopotamus minutus*, in Cyprus, is interesting in comparison with the dwarf species from Malta and Sicily, and because the presence of an extinct mammalian fauna in this locality had not previously been recorded. The occurrence of these different, though apparently closely related, races of small elephants in widely separated islands of the Mediterranean, lends probability to the theory that this is a case of independent development along similar lines, the result of similar circumstances and environments. Nevertheless, it would perhaps be wise not to take it for granted, without further evidence, that this diminutive size is wholly and entirely due to specialisation.

I hope shortly to be able to communicate a more detailed account, with figures and full descriptions, of this collection of elephant remains from Cyprus.

* 'Zool. Soc. Trans.,' vol. 6, Pl. 53, fig. 9, p. 295.

† Dr. Forsyth Major, "Die Tyrrhenis," 'Kosmos,' vol. 7, 1883, p. 7.

"On the Discovery of a Species of Trypanosoma in the Cerebro-Spinal Fluid of Cases of Sleeping Sickness." By ALDO CASTELLANI, M.D. Communicated by the Malaria Committee of the Royal Society. Dated "Entebbe, Uganda, 5th April, 1903." Received May 8,—Read May 14, 1903.

On the 12th November, 1902, when examining a specimen of cerebro-spinal fluid taken by lumbar puncture during life from a well-marked case of *sleeping sickness*, I was surprised to observe a living trypanosoma. Since that date I have made as many observations in this direction as possible, and the results are to my mind sufficiently surprising to excuse me for presenting this preliminary note.

These trypanosomes are not in large numbers, so that to find them it is necessary to draw off at least 15 c.cs. of the cerebro-spinal fluid. It is better to reject the first few c.cs. as they are apt to contain blood. When the fluid comes away clear, 10 c.cs. are collected and centrifuged for 15 minutes. At the end of this time there is found at the bottom of the tube a slight deposit of whitish sediment, and in some cases also a minute trace of blood.

The liquid above the sediment is poured off and the sediment examined under a moderately low power of the microscope. As the trypanosomes are at first fairly active they are easily detected.

The following tables represent the results of this investigation :—

Table I.—Sleeping Sickness Cases.

No.	Name.	Sex.	Age.	Date.	Stage of disease.	Microscopic appearance of sediment.	Presence of trypanosoma in cerebro-spinal fluid.	Remarks.
1	Mundo	M.	15	12/11/02	3rd	A few leucocytes, the majority of which are mononuclear. Some very rare red blood corpuscles	Present	
2	Maoli.....	M.	18	25/11/02	3rd	Some few leucocytes and R.B.C.	Absent	
3	Aritzo	M.	25	7/12/02	3rd	Some leucocytes. No R.B.C.	Absent	
4	Manika	F.	10	15/12/02	3rd	A few leucocytes. Very few R.B.C.	Present	Patient died on December 18, 1902. No complications. In fluid from lat. vent. Tryp. present.
5	Ialika	F.	22	15/12/02	2nd	A few leucocytes. Very few R.B.C.	Absent	
6	Asmeni	F.	8	15/12/02	3rd	Some leucocytes and many R.B.C.	Absent	
7	Bolenti	M.	10	22/12/02	3rd	Some leucocytes. No R.B.C.	Present	In fresh preparations of blood taken from a finger the same day I found a few trypanosomes apparently similar to those found in the lumbar puncture liquid, only their movements were apparently more lively.
8	A	M.	20	5/1/03	3rd	Few leucocytes and some R.B.C.	Absent	
				7/1/03	..	Some leucocytes and very few R.B.C.	Absent	

Table I —Sleeping Sickness Cases—*continued.*

No.	Name.	Sex.	Age.	Date.	Stage of disease.	Microscopic appearance of sediment.	Presence of trypanosoma in cerebro-spinal fluid.	Remarks.
9	Makassa	F.	25	25/1/03	3rd	Some leucocytes. No R.B.C.	Present	
10	Kaperi II	M.	14	25/1/03	2nd	A few leucocytes and very few R.B.C.	Absent	
11	Ally II	M.	30	2/2/03	3rd	Some leucocytes and R.B.C.	Absent	
12	Mocreza	M.	30	10/2/03	2nd	A few leucocytes and R.B.C.	Absent	
13	Budara	M.	22	27/2/03	2nd	Some leucocytes and very few R.B.C.	Present	
				2/3/03	..	Some leucocytes and very few R.B C.	Present	
14	Nombi	F.	30	27/2/03	1st	Few leucocytes and R.B.C.	Absent	
				24/3/03	2nd	Some leucocytes and R.B.C.	Absent	
15	Fatoma	F.	18	27/2/03	2nd	Very scarce leucocytes and no R.B.C.	Absent	
				4/3/03	..	Few leucocytes and R.B.C.	Absent	
				26/3/03	..	Few leucocytes and R.B.C.	Absent	
				1/4/03	..	Some leucocytes and a few R.B.C.	Present	
16	Zenabu	F.	22	24/3/03	1st	No R.B.C.	Absent	
				28/3/03	1st	Some R.B.C.	Absent	
				1/4/03	1st	Some RB.C.	Absent	

Table I.—Sleeping Sickness Cases—*continued.*

No.	Name.	Sex.	Age.	Date.	Stage of disease.	Microscopic appearance of sediment.	Presence of trypanosoma in cerebro-spinal fluid.	Remarks.
17	Benjamin	M.	20	25/3/03	2nd	..	Absent	
				28/3/03	..	Some few leucocytes and R.B.C.	Absent	
				29/3/03	..	Some few leucocytes and R.B.C.	Absent	
18	Zakibu	M.	25	25/3/03	2nd	Few leucocytes and R.B.C.	Present	
				27/3/03	2nd	Few leucocytes and R.B.C.	Present	
19	Seera	M.	25	26/3/03	2nd	..	Present	
20	Kimbra	M.	55	26/3/03	3rd	Some leucocytes. No R.B.C.	Present	
21	Abdulla	M.	..	26/3/03		No R.B.C.	Present	
22	Kagoya	F.	20	26/3/03	3rd	No R.B.C.	Present	
23	Keogaffum	M.	55	27/3/03	2nd	Some leucocytes. No R.B.C.	Absent	
				1/4/03		..	Absent	
24	Jacobo	M.	20	28/3/03	3rd	Some leucocytes. No R.B.C.	Present	The trypanosomes were much more numerous than in other cases.
25	Iegobaza	M.	40	27/3/03	2nd	Few leucocytes. No R.B.C.	Present	
26	Ibsarara	F.	35	27/3/03	3rd	Some leucocytes. No R.B.C.	Present	
27	Leobeni	M.	25	28/3/03	2nd	No. R.B.C.	Present	
28	Kidorme	M.	20	28/3/03	2nd	Some leucocytes and R.B.C.	Present	

Table I.—Sleeping Sickness Cases—*continued.*

No.	Name.	Sex.	Age.	Date.	Stage of disease.	Microscopic appearance of sediment.	Presence of trypanosoma in cerebro-spinal fluid.	Remarks.
29	Keagabidoia	M.	55	28/3/03	3rd	Some leucocytes and R.B.C.	Absent	
				1/4/03	3rd	Some leucocytes and R.B.C.	Absent	
30	Kitaroma	M.	25	28/3/03	2nd	Some leucocytes and R.B.C.	Absent	Only 5 c.c. of liquid collected.
				2/4/03	2nd	Some leucocytes and R.B.C.	Absent	
31	Waiswa	M.	10	29/3/03	2nd	Some leucocytes and R.B.C.	Present	
32	Kaperi I.	M.	8	23/3/03	3rd	No. R.B.C.	Present	Trypanosoma present also in the fluid taken from lateral vent. at the *post-mortem*.
33	Matasa	M.	28	29/3/03	2nd	Few leucocytes and R.B.C.	Absent	
				2/4/03	2nd	Few leucocytes and R.B.C.	Absent	
34	Decodeno	M.	25	31/3/03	2nd	Few leucocytes and R.B.C.	Present	

Table II.—Controls.

No.	Name.	Sex.	Age.	Date.	Disease.	Microscopic appearance of sediment.	Presence of trypanosoma.	Remarks.
1	Doanira	M.	35	11/1/03	Chronic nephritis	Very few leucocytes and R.B.C.	Absent	
2	Kano Buringo	M.	30	24/3/03	Trypanosoma fever	Very few leucocytes and R.B.C.	Absent	Dr. Baker found *Trypanosoma Gambiense* in blood some days ago.
3	Landu	M.	..	24/3/03	Enlargement of femoral glands	Few leucocytes. No R.B.C.	Absent	
4	Kamsuro	M.	..	28/3/03	Trypanosoma fever	Few leucocytes and R.B.C.	Absent	Dr. Baker found *Trypanosoma Gambiense* in blood the same morning.
5	Zake	M.	25	30/3/03	Cellulitis	Few leucocytes. No R.B.C.	Absent	
6	Scongo.....	..	..	30/3/03	Itch	Very few leucocytes and R.B.C.	Absent	
7	Pio.....	M.	12	30/3/03	..	Very few leucocytes and R.B.C.	Absent	
8	Kaperi III.....	M.	25	30/3/03	Itch	Very few leucocytes and R.B.C.	Absent	
9	Eliza.....	F.	18	30/3/03	Yaws	Very few leucocytes and R.B.C.	Absent	
10	Bofralour.....	M.	16	31/3/03	Pleuritis	Very few leucocytes and R.B.C.	Absent	
11	Zanabu II	F.	30	31/3/03	Headache	Very few leucocytes and R.B.C.	Absent	
12	Jordien Murjan...	M.	35	31/3/03	Trypanosoma fever.	Very few leucocytes and R.B.C.	Absent	Dr. Baker found *Trypanosoma Gambiense* in blood the same morning.

Table I shows that in 34 cases of sleeping sickness, the trypanosomes were found in the cerebro-spinal fluid taken by lumbar puncture during life in 20 cases, giving a rate of 70 per cent.

On two occasions I also examined in the same way fluid from the lateral ventricles and in both cases found the same parasite. In blood I found the trypanosoma once with certainty.

It may be thought that the trypanosomes are found in the cerebro-spinal fluid on account of the trace of blood which sometimes forms part of the sediment. But it will be seen from the table that in several cases there was no trace of blood.

Table II shows that in 12 cases of ordinary disease, the cerebro-spinal fluid taken during life by lumbar puncture, in no case contained trypanosoma, and it is important to note that 3 of these controls were cases of the usual trypanosoma fever, as described by Forde, Dutton, Manson, Daniels, &c.

Here it may be remarked that trypanosoma fever is by no means uncommon among the natives in Uganda, 3 cases having been met with, by Dr. Baker, one of the colonial surgeons here (Entebbe), within the last 3 weeks. I understand that Dr. Baker is publishing this most interesting observation. It must be clearly understood that these cases of trypanosoma fever bear no resemblance in their clinical features to sleeping sickness.

The trypanosoma found in the cerebro-spinal fluid of sleeping sickness does not as far as I have been able to make out differ materially in size and shape from the species one finds in the blood of trypanosoma fever, *Trypanosoma Gambiense* (Dutton), but possibly it is to be differentiated from this one, because in it, as a rule, the micro-nucleus lies nearer the extremity and the vacuole is apparently larger. Besides, its movements are not apparently so active, but this fact might be due to the effects of the centrifuge. In case it should prove to be a new species, the trypanosoma I have described might be called from the country where I have found it first—*Trypanosoma Ugandense.*

Relation of the Trypanosoma to Sleeping Sickness.

At the post-mortem examination of 80 per cent. of the cases where I found during life the trypanosoma, I grew from the blood of the heart and from the liquid of the lateral ventricles the variety of streptococcus I described many months ago in my first note. Up to that time I had never found the trypanosoma, but this is easily explained by the fact that I did not use the technique I have described in this note, viz., examination of a *large* quantity of liquid after long use of the centrifuge.

Influenced by my last investigations I would suggest as a working hypothesis on which to base further investigation that sleeping sickness

is due to the species of trypanosoma I have found in the cerebro-spinal fluid of the patients in this disease, and that at least in the last stages there is a concomitant streptococcus infection which plays a certain part in the course of the disease.

Note by the Secretary of the Royal Society.

As so far supporting the observations by Dr. Castellani recorded in the above communication, it may be desirable to state that Colonel Bruce, to whom in Uganda Dr. Castellani made known his discovery of the Trypanosoma, and who is now continuing the investigation begun by Dr. Castellani, has sent to the Royal Society a telegram, received May 4, stating that since Dr. Castellani left, in thirty-eight cases of sleeping sickness, he had found trypanosoma in every case in fluid obtained by lumbar puncture, and that he had found trypanosoma in the blood in twelve out of thirteen cases of sleeping sickness.

MICHAEL FOSTER.

"On Skew Refraction through a Lens; and on the Hollow Pencil given by an Annulus of a very Obliquely Placed Lens."

By J. D. EVERETT, F.R.S. Received January 22,—Read January 29, 1903. Received in revised form, April 20, 1903.

[PLATES 9 AND 10.]

PART I.—*Outline.*

1. The investigation here described was undertaken with the view of explaining the curious curves obtained by receiving on a screen, at certain distances, the hollow pencil which emerges from an annulus of a lens placed at large obliquity (such as 30° or 45°) to the incident beam.*

2. The first requisite is a process for calculating the direction-cosines of a ray refracted at a given surface, when those of the incident ray and of the normal are given, with the relative index of refraction. In the original computations for this paper, the method of procedure was, to calculate first the direction-cosines of the tangent to the refracting surface in the plane of incidence. A length unity along the refracted ray was then projected on this tangent and on the normal; and these two projections were themselves projected on the axes of co-ordinates, and added. It has subsequently been found better, instead of actually calculating the direction-cosines of the tangent, to eliminate them between two sets of equations, one of which is obtained by projecting the refracted ray in the manner above described, and the other by similar projection of the incident ray.

3. A single case of special simplicity is selected for the application of the process; the case of a narrow and thin annulus of a plano-convex lens, with a parallel pencil incident at 45° on its plane face, the index being 1·5. The annulus is supposed to be graduated, the division 0° being at the point furthest from the source, and the graduations being from 0° to ±180°. The plane containing the axis of the lens and the axis of the incident pencil passes through the divisions 0° and 180°, and is the only *plane of symmetry* of the system. For convenience of description, we shall suppose this plane to be vertical, the point 0° being the highest point of the annulus.

4. Twelve points 30° apart, starting from 0°, are taken on the annulus, as points of incidence; and the direction-cosines of the corresponding emergent rays are calculated to three or four places of decimals. The

* See S. P. Thompson, "Experiments on Zonal Aberration," 'Archives Néerlandaises,' 1901, and Fourth Traill Taylor Lecture, printed in the 'Almanac of the Brit. Journ. of Phot.' for 1903; also Alice Everett, "Photos. of Sections of Hollow Pencils," 'Jour. Brit. Astron. Assoc.,' vol. 13, 1902, pp. 74–75.

axis of the lens is taken as axis of z, the axis of y being in and the axis of x perpendicular to the plane of symmetry. The equations of the twelve emergent rays thus found enable us to plot twelve points of the section made by a plane at any given distance z; and through these a curve can be drawn by hand.

5. These sections are, however, inclined at about 45° to the emergent rays, and are about midway between the axes of z and y. To remedy this inconvenience, the equations of the rays are transformed to new axes (of η and ζ), the axis of ζ being midway between the axes of z and y, and coinciding with the original direction of the incident beam. Sections perpendicular to this direction are found by assigning different constant values to ζ; and a "direction-curve" is drawn, which is a section of a cone whose generators are parallel to the emergent rays.

6. Harmonic reduction is applied to the direction-cosines; and harmonic expressions containing either two sines without cosines or two cosines without sines are found to give a remarkably close representation of the facts.

7. Each ray of the emergent pencil intersects two other rays. One set of intersections are in the plane of symmetry, and are the intersections of rays symmetrically placed with respect to this plane. These intersections constitute the *secondary focal line*, which is absolutely straight, and lies in the production of the straight line drawn through the centre of curvature of the convex face of the annulus, parallel to the rays in the glass. These may be called "left-and-right" intersections.

8. The other set of intersections constitute the *primary focal line*. They may be described as "up-and-down" intersections, inasmuch as the plane of any pair of intersecting rays is at a small inclination to the vertical—that is, to the plane of symmetry; whereas the plane of a pair which intersect in the secondary line is perpendicular to the plane of symmetry. Each intersection involves an inversion of relative position of the two rays concerned; and the combined effect of the "up-and-down" inversion at the primary line, and the subsequent "left-and-right" inversion at the secondary, is to cause a distant section to be an inverted image of the annulus.

9. In the "up-and-down" intersections, the pairing of the rays follows an unsymmetrical law. Each of the rays between 0° and 79° is paired with a ray between 180° and 79°;—a fact which I discovered empirically in my original calculations. The exact law of pairing has since been detected by Professor A. E. H. Love. It is, that the chord joining a pair passes through a fixed point, namely the point in which the plane of the annulus is cut by the secondary line. See §§ 23, 24, 26.

10. The first rays to intersect are those from 0° and 180°; and their intersection is the vertex of the primary focal line. This line is approximately a parabola, lying in a plane which recedes with a

downward slope of about 1 in 6. Each end of the primary line is the intersection of two ultimately coincident rays from near 79°.

After a long interval, the intersections on the secondary line begin, the first being the intersection of rays from two points ultimately coincident at 180°, and the last the intersection of rays ultimately coincident at 0°.

11. In the cross-sections, of which numerous specimens at gradually increasing distances are given in Plate 9, every intersection of two rays, or (what is the same thing) every intersection of a ray with one of the two focal lines, appears as a double point, which is generally a point of crossing of two branches, but is sometimes a cusp; and in one instance the two double points coincide in a point of contact of two branches.

For the purpose of identifying individual rays, the numbers 0, 30, 60, &c., are marked, indicating the positions, in each section, of the rays which came from the points 0°, 30°, 60°, &c., in the right hand half of the annulus. They facilitate the tracing of reversals of position.

12. It is by no means a general law for oblique refraction through annuli that the primary crossings are completed before the secondary begin. More usually there is a large region in which the two crossings overlap—a circumstance indicated by the presence of three double points in a section, the middle one of the three being in the secondary and the two outer in the primary focal line.

13. Sections of pencils from annuli of obliquely placed lenses have been calculated by Steinheil* and by Finsterwalder,† the obliquity, however, being only 0° 48′ in Steinheil's calculations, and not exceeding 6° in Finsterwalder's. In both cases, the method of computation is that devised by Seidel‡ based on spherical trigonometry; and the calculations are only for the positions 0°, ± 45°, ± 90°, ± 135°, 180°.

Part II.—*Details.*

14. *General Process for Skew Refraction.*

Let l_1, m_1, n_1 be the direction-cosines of the normal,

l_2, m_2, n_2 those of the incident ray,

then, calling the angle of incidence χ, and the angle of refraction χ, we have

$$\cos \chi = l_1 l_2 + m_1 m_2 + n_1 n_2.$$

Hence, knowing the index of refraction, we can deduce sin χ' from sin χ.

* 'Munich, Akad. Sitz. Ber.,' vol. 19 (1889).

† 'Munich, Akad. Abhandl.,' vol. 17 (1892), p. 519.

‡ 'Munich, Akad. Sitz. Ber.,' 1866, p. 263.

Let $\lambda\ \mu\ \nu$ denote the direction-cosines of the tangent to the refracting surface in the plane of incidence.

Then $l_2\ m_2\ n_2$ may be regarded as the projections of a unit incident ray on the axes. But this unit ray gives a projection $\sin\chi$ on the tangent, and a projection $\cos\chi$ on the normal. Adding the projections of these on the axes, we have

$$\left.\begin{aligned} l_2 &= \lambda\sin\chi + l_1\cos\chi \\ m_2 &= \mu\sin\chi + m_1\cos\chi \\ n_2 &= \nu\sin\chi + n_1\cos\chi \end{aligned}\right\} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (1).$$

And in the same way, by projecting a unit refracted ray, we have

$$\left.\begin{aligned} l' &= \lambda\sin\chi' + l_1\cos\chi' \\ m' &= \mu\sin\chi' + m_1\cos\chi' \\ n' &= \nu\sin\chi' + n_1\cos\chi' \end{aligned}\right\} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (2),$$

l', m', n' denoting the required direction-cosines of the refracted ray.

Substituting the values of λ, μ, ν from (1), equations (2) become

$$\left.\begin{aligned} l' &= k(l_2 - l_1\cos\chi) + l_1\cos\chi' \\ m' &= k(m_2 - m_1\cos\chi) + m_1\cos\chi' \\ n' &= k(n_2 - n_1\cos\chi) + n_1\cos\chi' \end{aligned}\right\} \quad \ldots\ldots\ldots\ldots \quad (3)$$

k standing for $\sin\chi'/\sin\chi$ the relative index from the second medium to the first.

Equations equivalent to (3) are given in Herman's *Geometrical Optics*, §§ 18, 19 (Camb. Univ. Press, 1900) and have, I understand, been taught for many years by Mr. Webb at Cambridge.

15. In the original calculations for this paper, a clumsier method was used, in which $\lambda\ \mu\ \nu$ were computed by means of determinants.* The original results have been tested, and in some instances made more exact, by the use of equations (3).

* Since the tangent is coplanar with the normal and the incident ray, we have

$$A\lambda + B\mu + C\nu = 0,$$

where

$$A = \begin{vmatrix} m_1 & n_1 \\ m_2 & n_2 \end{vmatrix}, \quad B = \begin{vmatrix} n_1 & l_1 \\ n_2 & l_2 \end{vmatrix}, \quad C = \begin{vmatrix} l_1 & m_1 \\ l_2 & m_2 \end{vmatrix}.$$

Also, since it is perpendicular to the normal, we have $l_1\lambda + m_1\mu + n_1\nu = 0$.

Hence λ, μ, ν are proportional to the three determinants

$$L = \begin{vmatrix} B & C \\ m_1 & n_1 \end{vmatrix}, \quad M = \begin{vmatrix} C & A \\ n_1 & l_1 \end{vmatrix}, \quad N = \begin{vmatrix} A & B \\ l_1 & m_1 \end{vmatrix},$$

and λ, μ, ν are the quotients of these by $\sqrt{(L^2+M^2+N^2)}$. l', m', n' are then found by substituting the numerical values of λ, μ, ν in equations (2).

It can be shown, by expanding the determinants and making obvious reductions, that $\sqrt{(L^2+M^2+N^2)}$ is $\sin\chi$, and that the final results reduce to equations (3).

The Selected Case (*see* § 3).

16. The parallel rays incident at 45° on the first face, which is plane, are refracted into the lens so as to make with the normal (which is the axis of z) an angle of 28° 7½′. The sine of this angle is 0·4714, and its cosine 0·8819. The direction-cosines of the rays incident on the second or convex face of the lens are therefore—

$$l_2 = 0, \qquad m_2 = 0{\cdot}4714, \qquad n_2 = 0{\cdot}8819;$$

and are the same at all points.

Our calculations relate to a single narrow annulus of the convex face,* the axis of this annulus being the same as that of the lens. The radius of the annulus will be taken as the unit of length, and the centre of the annulus as the origin of co-ordinates.

The normals at all points are equally inclined to the axis of z, and we assume the sine of this inclination to be 0·1000; in other words, the radius of curvature is taken as ten times the radius of the annulus. This makes the cosine 0·9950, and the angle itself about 5° 44⅓′.

Let θ denote the angular distance of any point of the annulus from the summit (which is the farthest point from the source). Then the co-ordinates of the point are—

$$x_0 = \sin\theta, \qquad y_0 = \cos\theta, \qquad z_0 = 0,$$

and the direction-cosines of the forward-drawn normal at the point are

$$l_1 = \tfrac{1}{10}\sin\theta, \qquad m_1 = \tfrac{1}{10}\cos\theta, \qquad n_1 = 0{\cdot}9950.$$

From these we deduce, for the angle of incidence χ,

$$\cos\chi = l_1l_2 + m_1m_2 + n_1n_2 = 0{\cdot}04714\cos\theta + 0{\cdot}87749,$$

and $\sin\chi'$ is $\frac{3}{2}\sin\chi$; hence $\cos\chi'$ is known, and we have all the data for calculating the direction-cosines l' m' n' of the emergent ray by equations (3). The following values are thus found:—

	0°	30°.	60°.	90°.	120°.	150°.	180°.
l'	0	−0·0286	−0·0513	−0·0622	−0·0568	−0·0342	0
m'	0·650	0·658	0·677	0·707	0·740	0·766	0·777
n'	0·760	0·753	0·734	0·704	0·670	0·641	0·630

* The *annulus of the lens* which corresponds to this *annulus of the convex face* may have any thickness, but description is facilitated by supposing the thickness so small as to be negligible.

and the equations to an emergent ray are

$$\frac{x-\sin\theta}{l'} = \frac{y-\cos\theta}{m'} = \frac{z}{n'} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (4).$$

When the sign of θ is reversed, the sign of l' is reversed, and there is no change in m' or n'.

17. For transforming to the axes of $x\ \eta\ \zeta$ (see § 5), ζ being in the direction of the original beam incident on the first face, the required formulæ are—

$$\eta = (y-z)\sqrt{\tfrac{1}{2}}, \qquad \zeta = (y+z)\sqrt{\tfrac{1}{2}}.$$

$$l = l', \qquad m = (m'-n')\sqrt{\tfrac{1}{2}}, \qquad n = (m'+n')\sqrt{\tfrac{1}{2}},$$

l, m, n denoting the direction-cosines relative to the new axes. We thus find, for the seven selected points—

	0°.	30°	60°.	90°.	120°.	150°.	180°.
m	−0·0778	−0·0672	−0·0403	+0·00177	+0·0495	+0·0880	+0·1040
n	0·997	0·998	0·998	0·998	0·997	0·995	0·995
$\eta_0 = \zeta_0$ ..	0·7071	0·6124	0·3536	0	−0·3536	−0·6124	−0·7071

and the equations of an emergent ray are reduced to

$$\frac{x-\sin\theta}{l} = \frac{\eta-\eta_0}{m} = \frac{\zeta-\zeta_0}{n} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (5),$$

where $\eta_0 = \zeta_0 = \sqrt{\tfrac{1}{2}}\cos\theta$.

These give the values of x and η for a section at any distance ζ from the origin. The curves in Plate 9 have been obtained in this way, twelve points being plotted and a smooth curve drawn through them.

Description of Plate 9. (*See* § 11.)

18. The sections are arranged in increasing order of the distance ζ from the centre of the annulus.

The first row consists of sections nearer than the primary focal line. The first of them is at distance 4 (radii of the annulus) and is much flattened at the bottom owing to the large upward deviation of the lowest rays. When we pass to the next section, at distance 6, the lower side has become reentrant, and, as the distance increases, the lower side becomes more concave, the upper becoming at the same time less convex, till at $\zeta = 7{\cdot}25$ they form sensibly parallel arcs. At 7·5 the middle has become the narrowest part.

The second row consists of sections through the primary focal line.

It commences (at distance 7·72) with contact of the two branches, their common tangent being also a tangent to the primary focal line. The lower branch then passes through the upper, giving three areas separated by two points of self-cutting. These two points then travel outwards, causing the two loops beyond them to become smaller, till, at distance 8·51, the loops have vanished and are replaced by cusps, which are at the two ends of the primary focal line.

The third row consists of sections intermediate between the two focal lines, the cusps having changed to rounded off angles, and one of the sections (at distance 11) being approximately an equilateral triangle with corners rounded off. The last of them, at distance 13, has a fairly sharp angle at the top.

The fourth row consists of seven sections through the secondary line and one at a greater distance. The first has a cusp at the top, changing into a small loop, and then we have figures of 8, with the upper loop enlarging and the lower diminishing, till at distance 18·51 the lower loop has been replaced by a cusp. There is thus a cusp at each end of the secondary (as well as of the primary) focal line. When the figures are produced experimentally by receiving the pencil on a card, it is possible to hold the card in a very oblique position in which the two sides of the figure 8 unite, and give a single line resembling the image of a slit. This image is the secondary focal line.

When the secondary line is passed, the area of the section increases rapidly. The form is at first that of an oval sharpened at the bottom, as shown in the last figure of the Plate; but, as the distance increases, the two ends become nearly alike; and at still greater distances, the lower half becomes broader and blunter than the upper, as exemplified in the section at infinite distance which stands first in Plate 10.

19. In the figures of Plate 9, the zero point $x = 0$, $\eta = 0$, through which an undeviated ray from the origin would pass, is indicated by a large dot, and it always lies below the centre of figure—a consequence of the preponderating action of the lower portion of the annulus. At distances 6 and 9·9 the zero point coincides with the 180° point and the 0° point respectively, and at intermediate distances it *lies outside the curve*. This explains the observed fact that, when two concentric annuli are employed, differing greatly in size, the curve due to the inner often lies outside that due to the outer. In dealing with the solid pencil given by a very obliquely placed lens, it is clearly not permissible to identify the bounding surface of the pencil with the surface constituted by rays from the outermost zone.

20. The direction-cosines l', m', n', are approximately simple-harmonic functions of the angular co-ordinate θ of the point of emergence. The amplitude of the chief term (of period 360°) is some fifteen or twenty times as great as that of the next term (of period 180°); and when

both these terms are included, the largest departures from agreement with the twelve data amount to about $\frac{1}{3000}$ of the chief amplitude for l', and $\frac{1}{700}$ for m' and n'. The harmonic expressions to which these remarks apply are

$$l' = -0{\cdot}0624 \sin\theta + 0{\cdot}0032 \sin 2\theta,$$
$$m' = 0{\cdot}7103 - 0{\cdot}0628 \cos\theta + 0{\cdot}0033 \cos 2\theta,$$
$$n' = 0{\cdot}6997 + 0{\cdot}0645 \cos\theta - 0{\cdot}0047 \cos 2\theta,$$

from which are deduced

$$m = 0{\cdot}0075 - 0{\cdot}0900 \cos\theta + 0{\cdot}0057 \cos 2\theta,$$
$$n = 0{\cdot}9970 + 0{\cdot}0013 \cos\theta - 0{\cdot}0010 \cos 2\theta.$$

The maximum error for m is about $\frac{1}{90}$ of the amplitude.

As regards n, the largest departure from agreement is about $\frac{1}{20000}$ of the large constant term 0·9970, and therefore does not materially affect the values of x and y calculated from

$$x = \sin\theta + \frac{l}{n}(\zeta - \zeta_0), \qquad \eta = \eta_0 + \frac{m}{n}(\zeta - \zeta_0),$$

in accordance with equations (5).

21. By plotting the values of l and m as co-ordinates, we obtain the *direction-curve*—the large outer curve which stands first in Plate 10. It is a section of the *direction-cone*, that is of a cone having its vertex at the origin and its generators parallel to the emergent rays. The large dot in the centre of the inner curve is the point in which the section is cut by the axis of ζ, and indicates the *direction of no deviation*. The plane of deviation for any ray passes through this point; and the radius vector drawn from this point is proportional to the tangent of the total deviation. The direction-curve may be otherwise called the *section at infinite distance*, being the form to which the section tends as the distance is increased.

The small inner curve represents the annulus as projected on the plane of the section, the scale being half that of Plate 9. The seven selected points in the positive half of the annulus are marked with their values of θ; and a comparison of these with the markings of θ on the outer curve shows the inversion which each ray undergoes, both up-and-down and left-and-right.

Crossing of Rays in the Secondary Focal Line.

22. The rays from two symmetrically placed points $\pm\theta$ meet in the plane of symmetry; and the locus of these meetings is the *secondary focal line* of the hollow pencil. Five of them can be found by putting

$x = 0$ in equations (4) or (5) of §§ 16, 17, using the values of l', m', n', or of l, m, n, there given for $\theta = 30°, 60°, \ldots 150°$. The two which correspond to the points 0° and 180° can in like manner be found by applying equations (3) to values of θ indefinitely near to 0 and π respectively.

Near 0° we thus obtain, for small θ,

$$\frac{x-\theta}{-0{\cdot}05662\theta} = \frac{y-1}{0{\cdot}6505} = \frac{z}{0{\cdot}7595};$$

and near 180°, for $\pi - \phi$ with ϕ small,

$$\frac{x-\phi}{-0{\cdot}06966\phi} = \frac{y+1}{0{\cdot}7768} = \frac{z}{0{\cdot}6297},$$

which, for $x = 0$, give

$\theta = 0°$,	$y = 12{\cdot}50$, $z = 13{\cdot}43$	$\eta = -0{\cdot}66$, $\zeta = 18{\cdot}34$.*
$\theta = 180°$,	$10{\cdot}15$, $9{\cdot}04$	$0{\cdot}785$, $13{\cdot}57$.

The two points thus determined are the ends of the secondary focal line, and the other five points when plotted are found to be sensibly in the straight line joining these two. The tangent of the inclination of the line to the axis of ζ, as computed from the co-ordinates $\eta\zeta$ of its ends, is $\frac{1{\cdot}445}{-4{\cdot}77} = -{\cdot}303$.

Sections of the pencil made through the secondary line, are figures of 8 (see 4th row of Plate 9); the crossing point of the 8 being the point in which the line meets the section. At the ends of the line, one loop of the 8 vanishes and is replaced by a cusp. The cusps are shown separately with tenfold magnification, in Plate 10.

23. A simple application of descriptive geometry suffices to show that, in every case of refraction of a homocentric pencil at a spherical surface, all the refracted rays pass through a straight line, namely, the straight line which joins the point-source S to the centre of curvature C of the refracting surface. For, if P be the point of incidence on this surface, PC is the normal, and SPC the plane of incidence. The refracted ray lies in this plane, and therefore meets the line SC. In the case with which we are dealing, S is at infinity, and SC is parallel to the rays within the lens.

24. Again, the plane SCP cuts the annulus in a second point P′, and the plane of incidence SCP′ is identical with SCP. The refracted rays at P and P′ lie in this plane, and their intersection is a point in the primary focal line.

* It is interesting to compare the distance 18·34 of the further end of the secondary line with the focal length of the annulus, which is 19·78.

25. Applying these principles, we have

$$\text{Deviation at first refraction} = 45° - 28°\ 7\tfrac{1}{2}' = 16°\ 52\tfrac{1}{2}',$$

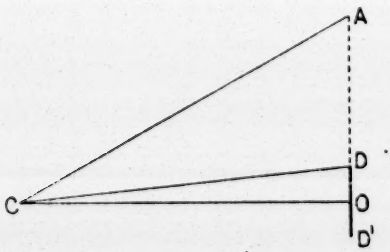
which is accordingly the downward slope of the rays in the glass. The tangent of $16°\ 52\frac{1}{2}'$ is 0·303, which agrees with the value above deduced from the co-ordinates of the ends of the secondary line.

Again, the distance OC of the centre of curvature from the origin is $\sqrt{99} = 9{\cdot}950$; and the condition that C is collinear with the ends of the secondary line is the vanishing of the determinant.

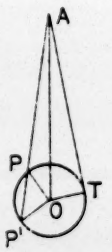
$$\begin{vmatrix} 12{\cdot}50, & 10{\cdot}15, & 0 \\ 13{\cdot}43, & 9{\cdot}04, & -9{\cdot}95 \\ 1, & 1, & 1. \end{vmatrix}$$

Expanding, we find that the positive terms amount to 237·4, and the negative to 237·3, a sufficiently close agreement.

26. Let A in the annexed figures be the point where a line through C parallel to the rays in the lens meets the plane of the annulus. If we draw any straight line through A, cutting the annulus in two points P, P′, the rays at these two points are in the same plane of incidence



OD = OD′ = radius of circle = 1.
OA = 5·32 in both figures.
DC = 10, giving CO = 9·95.



AT is a tangent.
APP′ any secant.

CAPP′, and will therefore meet; the point in which they meet being in the primary focal line. P and P′ coincide at the point of contact T of a tangent from A, and the ray from T passes through one end of the primary focal line, its other end being symmetrically placed on the other side of the plane of symmetry.

We have $OC = \sqrt{99}$, $OA = OC \tan 28°\ 7\frac{1}{2}' = 5{\cdot}318$, and the angular position θ of T is determined by $\operatorname{cosec}\theta = OA$, giving $\theta = 79°\ 10'$. The rectangle AP. AP′, or the square of AT, is 27·2847. For any given value of $\theta = AOP$ we can, from these data, calculate $\theta' = AOP'$. The following pairs of points of emergence of intersecting rays are thus found—

θ	0°	30°	60°	90°	120°	150°
θ'	180°	136° 10′	99° 35′	68° 40′	43° 4′	20° 44′

besides $\theta = \theta' = 79° 10'$.

27. To trace the primary focal line, I have calculated, by formulæ (3), the equations of the rays (in terms of $x\,y\,z$) at these thirteen points, and plotted on a large scale the sections of the rays by planes of constant z, the smallest value of z being 5·304, which corresponds to the intersection of the rays from 0° and 180°, the largest 6·05, which corresponds to 79° 10′, and the others being 5·4, 5·5, 5·6, 5·7, 5·8, 5·9, 6·0. Some of these sections* are reproduced in Plate 10. The co-ordinates $x\,y$ of the points of self-cutting of the curves (drawn carefully by hand through the plotted points) were adopted as the co-ordinates of points of the primary line; and the results are exhibited in Plate 10 in the shape of three curves which are the projections of the primary line on the co-ordinate planes of $x\,\eta\,\zeta$. These projections are on the same scale as the curves in Plate 9. The scale of the intersecting curves is five times as large. The projection on the plane of $\eta\,\zeta$ (the plane of symmetry) is very nearly a straight line. The other two projections show that the form of the primary line is approximately parabolic. The length of the chord joining its ends is 0·952, and the distance of this chord from the vertex, 0·911. The tangent of the slope of the approximate plane of the curve is about $-\frac{1}{6}$; and the ends of the curve (which are its lowest points) are just above the plane of $x\,\zeta$.

28. A general view of the system as projected on the plane of symmetry is given (on one-fourth the scale of Plate 9) at the foot of Plate 10. The highest and lowest points of the annulus are marked 0 and 180, and the rays incident at these two points are traced as far as their meeting with the secondary focal line. C is the centre of curvature of the convex face of the lens, the radius of curvature being ten times the radius of the annulus. CA, meeting the plane of the annulus in A, is parallel to the rays within the lens, and its production coincides with the secondary focal line.

Cusps in the Sections.

29. Every cross-section through an end of a focal line contains a cusp, which is the transition from a small loop to a rounded-off angle in sections taken near it. Three such sections, either of constant z or of constant ζ can be taken; namely, one section through each end of the secondary, and a single section through both ends of the primary.

30. The values of $dx/d\theta$ and $dy/d\theta$ for constant z, and of $dx/d\theta$ and $d\eta/d\theta$ for constant ζ, vanish at a cusp, and are very small in the region

* The markings 21, 43, 69, 100, 136, 79, against the curves are abbreviations for 20° 44′, 43° 4′, 68° 40′, 99° 35′, 136° 10′, 79° 10′.

around it. This property is of great assistance in locating the ends of the focal lines. Physically interpreted, it indicates close aggregation of rays, and consequent increase of luminous intensity, at the ends of the two focal lines.

31. To find at what distant z from the plane of the annulus the values of x and y for the ray at 0° are stationary for changes of θ.

We have in general

$$x = \sin\theta + \frac{l'}{n'}z; \qquad y = \cos\theta + \frac{m'}{n'}z.$$

Hence, for constant z,

$$\frac{dx}{d\theta} = \cos\theta + \frac{z}{n'^2}\left(n'\frac{dl'}{d\theta} - l'\frac{dn'}{d\theta}\right),$$

$$\frac{dy}{d\theta} = -\sin\theta + \frac{z}{n'^2}\left(n'\frac{dm'}{d\theta} - m'\frac{dn'}{d\theta}\right).$$

Differentiating the harmonic expressions (§ 20) for l' m' n', we see that the expressions for $dm'/d\theta$ and $dn'/d\theta$ contain only sines, and consequently vanish at 0°. Hence $dy/d\theta$ vanishes identically at 0°, and $dx/d\theta$ reduces to $\cos\theta + \frac{z}{n'}\frac{dl'}{d\theta}$, which is to vanish.

This gives $1 - \frac{560}{7598}z = 0$, $z = 13{\cdot}57$, agreeing fairly with the direct determination $z = 13{\cdot}43$.

In like manner, for the ray at 180°, the conditions reduce to $-1 + \frac{688}{6308}z = 0$, $z = 9{\cdot}17$; to compare with the direct determination 9·04.

These two cusps are shown (on ten times the scale of Plate 9) at the top of Plate 10.

32. In the case of the cusps at the ends of the primary line, which are known to be on the rays from ±79° 10′, direct application of formulæ (3) to the values 78°, 79°, 80°, shows that, for x and y to have the same values approximately for all three, z must be approximately 6. More exactly, the equations of the ray at 79° 10′ are—

$$x = 0{\cdot}98218 - 0{\cdot}08371z, \qquad y = 0{\cdot}18795 + 0{\cdot}97178z;$$

and those of the 79° ray,

$$x = 0{\cdot}98163 - 0{\cdot}08359z, \qquad y = 0{\cdot}19081 + 0{\cdot}97131z.$$

The value $z = 6{\cdot}05$ gives, for both rays,

$$x = 0{\cdot}476, \qquad y = 6{\cdot}0672.$$

Transforming from y and z to η and ζ, we have

$$\eta = 0{\cdot}012, \qquad \zeta = 8{\cdot}568.$$

The form of these cusps, for a section of constant z, is exhibited near the centre of Plate 10, on half the scale of the two cusps of the secondary (which are for sections of constant ζ).

33. The following examples illustrate the use of the harmonic formulæ:—

(*a*) To find the points of the annulus at which the deviation is exactly perpendicular to the plane of symmetry—

We must put $m = 0$, giving

$$900 \cos\theta - 57 \cos 2\theta = 75, \text{ whence } \theta = \pm 88^\circ\ 51'.$$

(*b*) To find the points at which the horizontal deviation is greatest, we must put $dl/d\theta = 0$, giving

$$\frac{\cos 2\theta}{\cos\theta} = \frac{624}{64} = 9\tfrac{3}{4}, \qquad \theta = \pm 95^\circ\ 46'.$$

(*c*) For the total deviation to be a maximum or minimum, its cosine n must be a minimum or maximum. We thus obtain—

$$\frac{dn}{d\theta} = -0{\cdot}0013 \sin\theta + 0{\cdot}0020 \sin 2\theta,$$
$$= -0{\cdot}0001 \sin\theta\,(13 - 40\cos\theta) = 0.$$

The factor $\sin\theta$ vanishes at 0° and 180°, at which there are maxima. The factor $13 - 40\cos\theta$ vanishes for $\theta = \pm 71^\circ$, and gives the points of minimum deviation.

The points on the "direction curve" which correspond to these three determinations are indicated by the words "horizontal," "maximum," "minimum," on the right hand of the curve.

(*d*) To find at what distance z the radius of curvature of a cross section becomes infinite at one of the two points 0° and 180° in the section.

From equations (4) we deduce

$$\frac{dx}{d\theta} = \cos\theta + z \frac{d}{d\theta}\frac{l'}{n'},$$

$$\frac{dy}{d\theta} = -\sin\theta + z \frac{d}{d\theta}\frac{m'}{n'}.$$

At 0° and 180°, $dx/d\theta$ is finite, but $dy/d\theta$ vanishes (since $dm'/d\theta$ and $dn'/d\theta$ contain only sines). Hence the tangent is horizontal. The condition of an infinite radius of curvature is $d^2y/d\theta^2 = 0$; or

$$-\cos\theta + z \frac{d^2}{d\theta^2}\frac{m'}{n'} = 0.$$

For the 0° ray this is

$$\frac{1}{z} = \frac{d^2}{d\theta^2}\frac{m'}{n'} .$$

and for the 180° ray

$$\frac{1}{z} = -\frac{d^2}{d\theta^2}\frac{m'}{n'} .$$

We have

$$\frac{d}{d\theta}\frac{m'}{n'} = \frac{1}{n'}\frac{dm'}{d\theta} - \frac{m'}{n'^2}\frac{dn'}{d\theta} ,$$

$$\frac{d^2}{d\theta^2}\frac{m'}{n'} = \frac{1}{n'}\frac{d^2m'}{d\theta^2} - \frac{m'}{n'^2}\frac{d^2n'}{d\theta^2} + \&c.,$$

the &c. consisting of terms which vanish at 0° and 180°. We thus obtain at 0°, $z = 8{\cdot}554$, and at 180°, $z = 3{\cdot}530$.

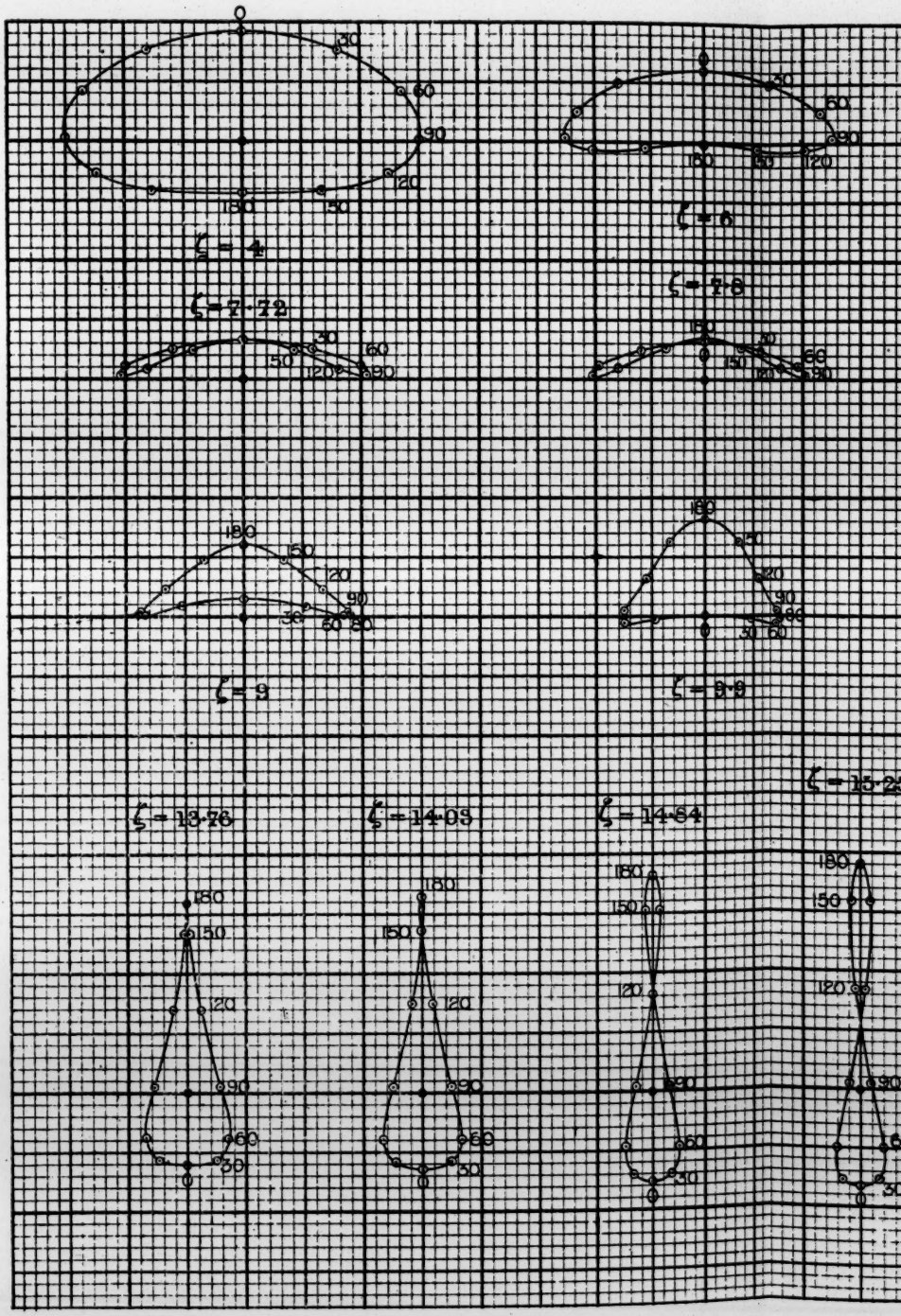
As the curves are symmetrical about the axis of y, the first differential coefficient of y that does not vanish must be of even order. It is accordingly of the fourth order at the two points thus determined; and the curve is sensibly straight for a considerable distance.

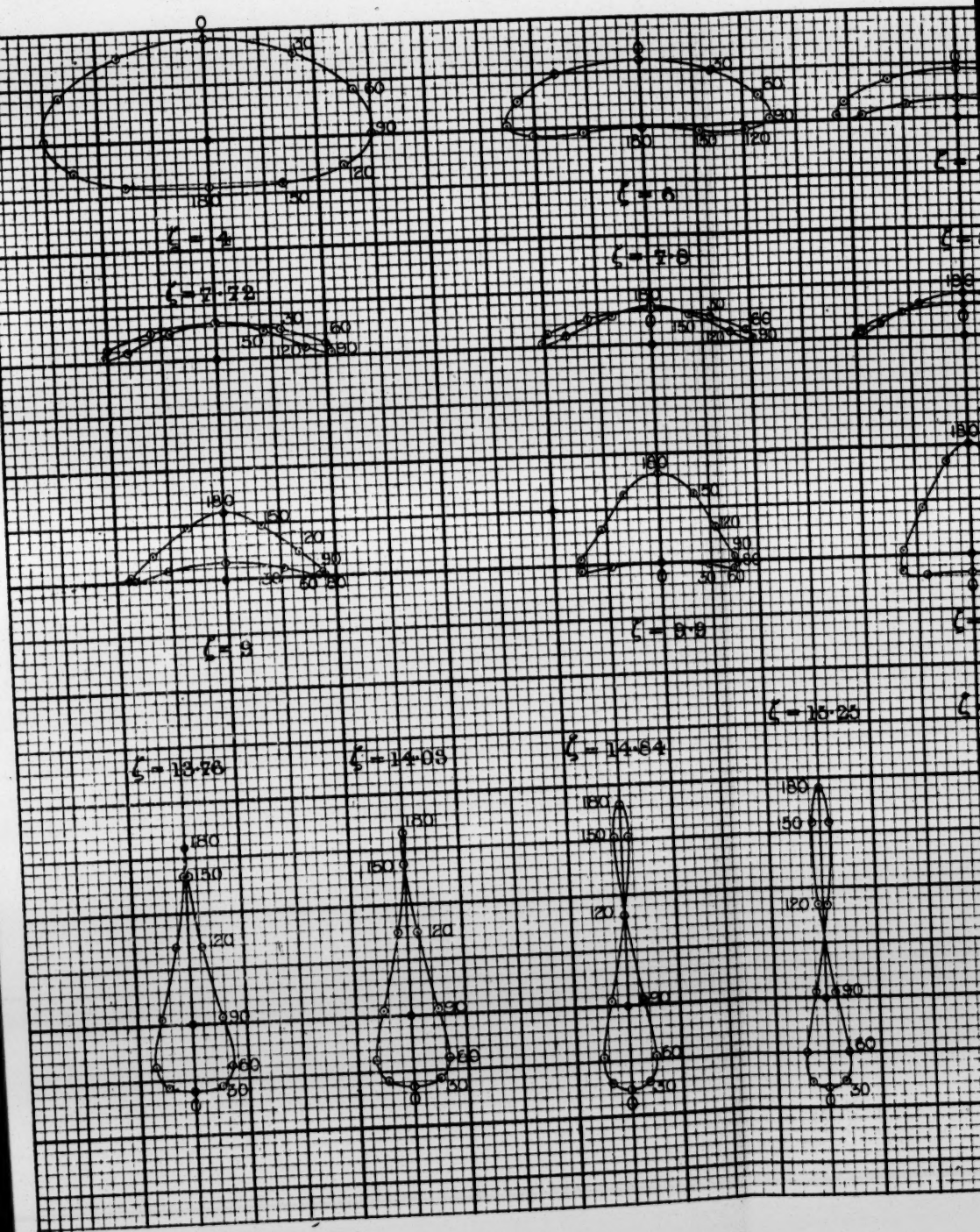
If, instead of sections of constant z, we take sections of constant ζ through these two points, the conclusions remain true; for, by Meunier's theorem, the radii of curvature in the two sections are in a finite ratio. From the above values of z, together with the equations of the rays, we can deduce the values of y, and then transform to $\eta\ \zeta$. We thus find—

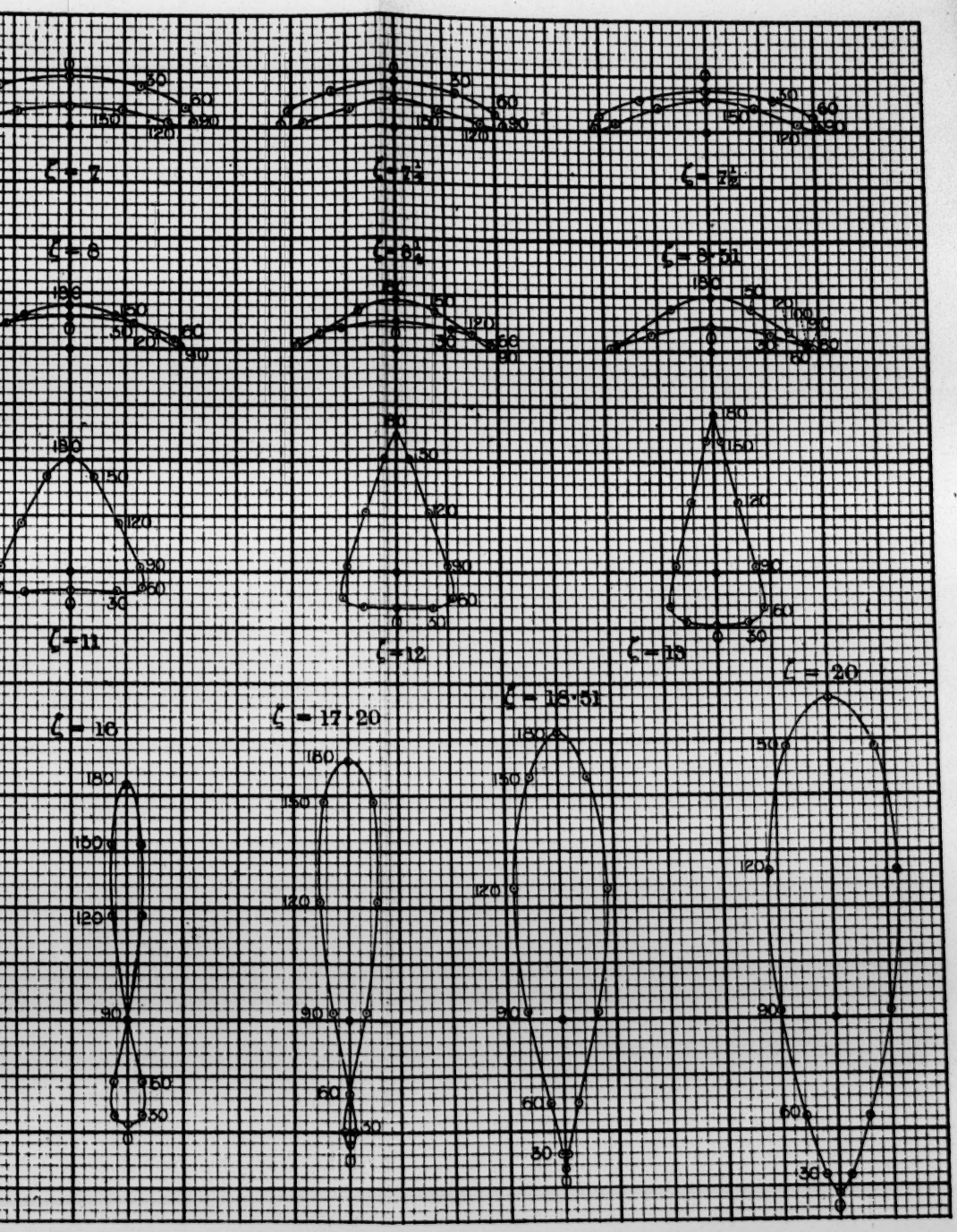
$$\text{for } \theta = 0°,\ \zeta = 11{\cdot}94, \qquad \text{for } \theta = 180°,\ \zeta = 4{\cdot}86,$$

results which are illustrated by the sections for

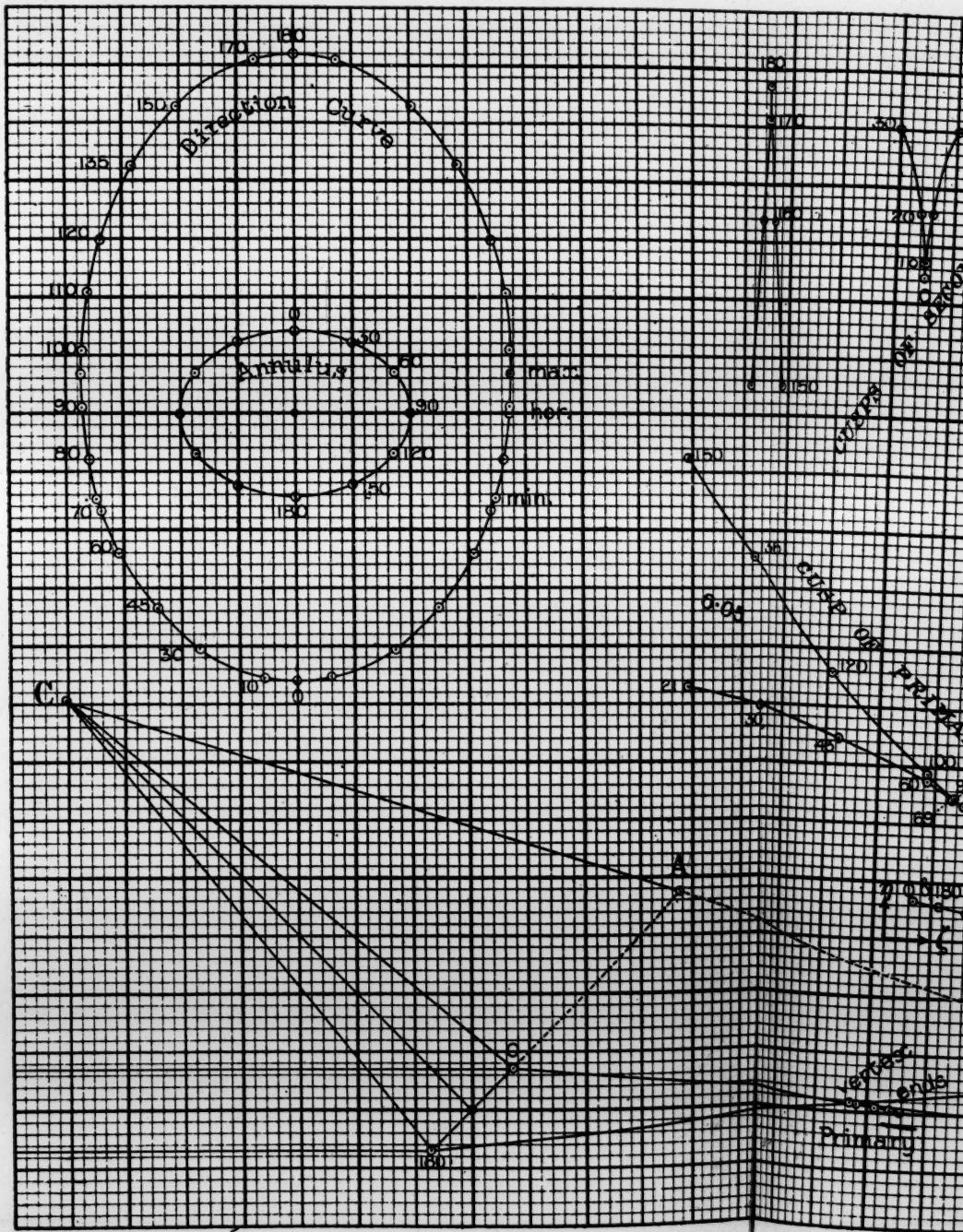
$\zeta = 12$, $\zeta = 4$, and $\zeta = 6$, in Plate 9.

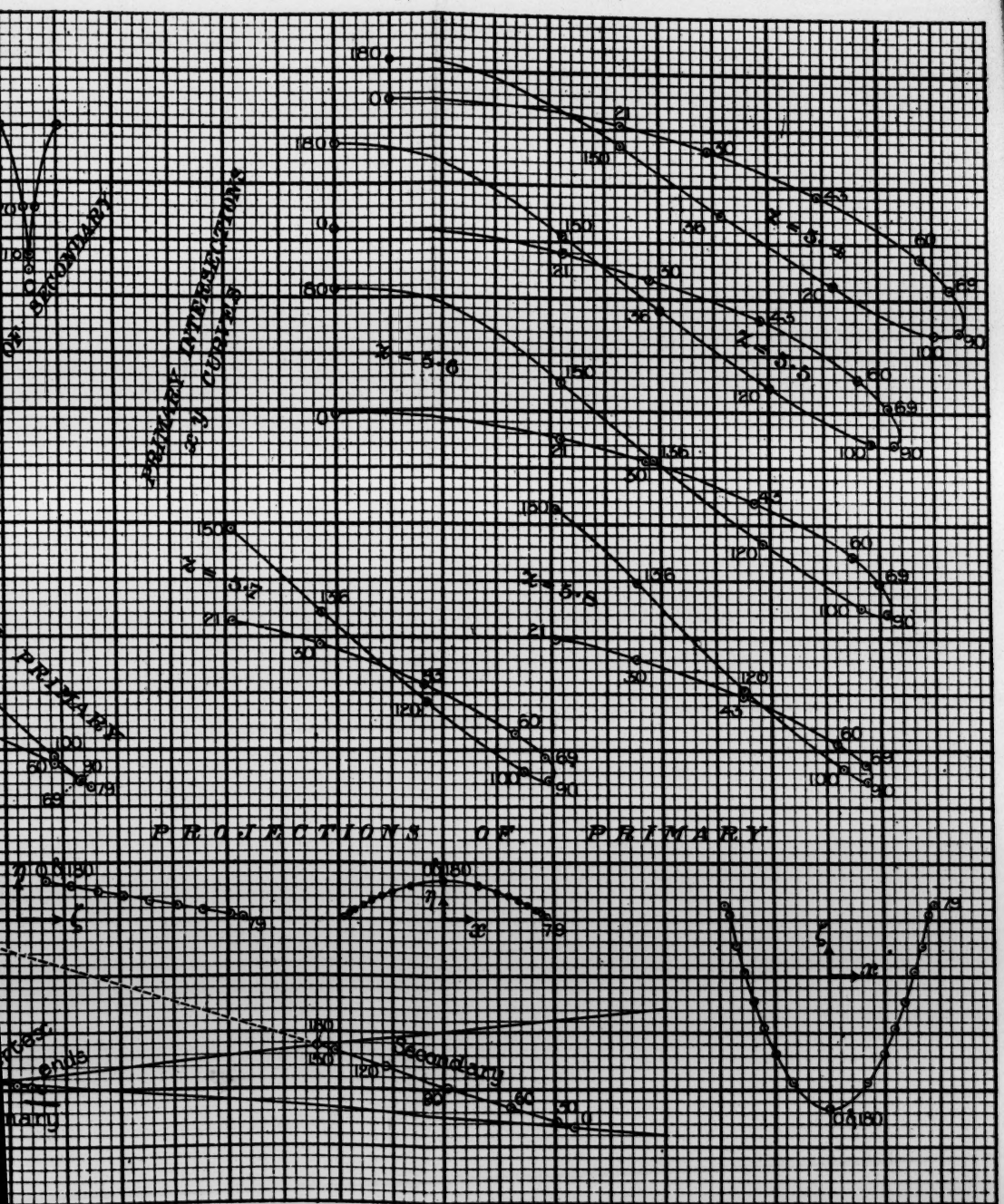

ζ = 4
ζ = 6
ζ = 7·72
ζ = 7·8
ζ = 9
ζ = 9·9
ζ = 13·76
ζ = 14·03
ζ = 14·64
ζ = 15·2


ζ = 4
ζ = 7·72
ζ = 9
ζ = 6
ζ = 7·6
ζ = 9·8
ζ = 13·76
ζ = 14·03
ζ = 14·64
ζ = 16·25


ζ = 7
ζ = 7½
ζ = 7½
ζ = 8
ζ = 8½
ζ = 8·51
ζ = 11
ζ = 12
ζ = 13
ζ = 16
ζ = 17·20
ζ = 18·51
ζ = 20

Everett.




PROJECTIONS OF PRIMARY
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